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ABSTRACT

The document introduces a project to develop course packages integrating innovative and conventional economics teaching techniques. The major objective of the project was to improve productivity in economics teaching at the college level. The document is presented in four sections. Section I explains that the project was initiated in 1976 by TEFRC (economics research center at Heriot-Watt University, Edinburgh, Scotland). Various phases of the project are identified: development of economics course packages, evaluation of materials, and follow-up studies of learning processes and methods of diffusing educational technology. Section II describes a pilot study in which 570 college students in economics courses worked with two course packages. Information is presented on working out logistics of the experiment, improving materials as a result of the pilot study, and design improvements for the final course package. Section III reviews classical principles of experimental design and identifies problems involved in creating experimental designs. Problems include failing to consider all relevant alternatives, dealing with variables which are difficult to observe, and selecting procedures which are optimal in a given situation. The final section presents a directory of institutions which participated in the study. (DB)

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Economic Evaluation of New Technologies in Higher Education.

N.I.E. Report Phase 1,

Volume 1 of 7

March 1978

THE ESMEE FAIRBAIRN RESEARCH CENTRE, HERIOT-WATT UNIVERSITY, EDINBURGH.

SP 011 933

The Esmée Fairbairn Research Centre, TEFRC, was established in 1970 and grew out of the Economics Education Project which was concerned with analysing how economics was being taught in schools, polytechnics, colleges and universities in the United Kingdom. Since its establishment, the Centre has undertaken several studies in the United Kingdom and United States of America, focussing mainly on efficiency in higher education.

Beginning in 1975, the principal research activity of TEFRC will be evaluating the cost effectiveness of alternative systems of instruction in higher education and assessing the likelihood of their diffusion in different institutional settings.

The Research Centre is part of the Department of Economics at Heriot-Watt University. The Director is Professor Keith G Lumsden.

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PREFACE

In 1975 Professors Richard E Attiyeh, Allen C Kelley and Keith G Lumsden submitted a proposal 'Economic Evalution of New Technologies in Higher Education' Volumes I and II, to the National Institute of Education to fund a research project. The project was designed to evaluate the cost-effectiveness of alternative systems of instruction in higher education and to assess the likelihood of their diffusion in different institutional settings.

The project, which is of six years duration and which is in three phases was approved in August, 1976. An interim report was submitted to the National Institute of Education in September, 1977. This report is in fulfillment of the first phase of the project.

March 1978

Keith G Lumsden
Project Director

1.1 Introduction

In August 1976 TEFRC began work on the first phase of the NIE Project which has as its ultimate objective the improvement of productivity in teaching in higher education, and which has as its more immediate objective the improvement of productivity in economics teaching in colleges and universities. There are three distinct phases constituting the scope of work for the entire project.

The first phase involved the development of course packages which integrate innovative and conventional teaching techniques. The second phase is concerned with the evaluation of these course packages in terms of their cost effectiveness by means of a large scale experiment. The third and final phase consists of analysis of the experimental data and follow up studies of participating institutions to shed light on both the educational process by which learning takes place and the diffusion process by which educational technology is adopted.

The first phase of the project, which was carried out in two stages, had as its objective the development of three complete course packages, including accompanying measuring instruments and the testing of these packages in a pilot study in three universities. Excluding a conventional micro package and a conventional macro package constructed for comparison purposes, five new packages, three in microeconomics and two in macroeconomics, have been developed. Two of these packages have been tested in the pilot study and a third is being tested currently.

While definitive results require the strict experimental controls planned for phase two of the project, and while caution must be exercised in extrapolating preliminary findings, the results of the pilot study are of such magnitude that it can be predicted confidently that for some of the courses packages not only can learning be increased significantly, but concurrently course costs can be reduced drastically.

1.2 The Course Content Outlines

A course package consists of a combination of various teaching techniques integrated into a programmed system of instruction. To compare the effectiveness of different course packages it is essential that each package be designed in accordance with the same educational objectives. That is, each package must be planned and utilized to teach the same body of knowledge. Preparatory to designing the material which makes up the course packages, it was necessary to work out detailed outlines of the course content both for microeconomics and macroeconomics. These outlines, contained in Volumes 3 and 4, serve also as an indispensable guide to teachers who participate in the experiment in the preparation of lectures.

Both the micro and macro courses were broken down into nine units, each of which treats a major topic of the typical introductory course. Throughout the outlines, terminology and analytical techniques are employed which will be consistent with those utilized in examinations and other measuring instruments. Consequently, all students participating in the experiment, regardless of which course package they are taught under, will be exposed to a common approach to the subject matter. ONLY IF THIS IS TRUE will it be possible to interpret differences in performance to differences in course packages rather than to differences in course content.

Development of the course outlines represents a major undertaking because of the substantial variation in course content which characterizes existing curricula throughout the United States. At a superficial level there is no question that there is a broad consensus as to what should be taught in the principles course. Once the problem of trying to identify that consensus in detail is confronted, however, it becomes clear that the consensus is more apparent than real. Students are exposed to widely differing terminology, levels of sophistication, and coverage of topics. While diversity may in itself be a desirable end for this project, it poses a serious problem which the course outlines are designed to overcome. In order to meet this objective, it has been necessary to organize and detail the core content of the two courses in a manner which is consistent with the views of the principal investigators and which strikes a balance among the varying views reflected in existing curricula. This has proved to be a time-consuming process. The detailed specification of a core of basic economics still leaves considerable room for an individual instructor adding topics and materials when the core has been completed.

The careful delineation of the intellectual content of the core material of the principles course will serve another useful purpose. A review of existing teaching practices has made clear that a good deal of the observed variation stems from an apparent inadequate understanding of or inability to articulate basic economics principles on the part of many college and university teachers. In part, this may be a reflection of a disinterest, rather than an ineptitude, for teaching. Whatever the root cause, however, the course outlines as they have been constructed will provide the less able or less willing economics teachers with a means to improving their performance, over and above any benefit associated with the other aspects of the course packages being developed.

1.3 Innovative Teaching Techniques

The three innovative teaching techniques which are to be integrated with conventional methods of instruction into new course packages are a computerized course management system (TIPS), computer simulation games, and case studies. Each of these techniques, described in detail in the initial proposal to N.I.E., had undergone considerable development - largely due to the impetus provided by the project's principal investigators - prior to the beginning of the project. Never, however, had they been integrated systematically with course packages designed to take advantage of the particular strengths of each technique. Nor had they been developed to be used with the specific course outlines which have now been constructed as part of this project. Consequently, assembly of the course packages has required considerable redesign of previously existing versions of these techniques and the development of even more new materials to meet the requirements of the course packages as they have been conceived.

1.3.1 TIPS - the Teaching Information Processing System

TIPS provides a means of designing and coordinating teaching so as to utilize a number of teaching techniques and instruments. The individual student is provided with instruction designed to meet his or her separate needs. Through TIPS, the degree of individualized instruction is largely invariant to class size.

At the completion of each core topic, students are given a "survey" designed to measure their progress, level of attainment, and areas of misunderstanding. These surveys take the form of multiple choice questions on one or more topics or "concepts". For each concept three to five questions are used, resulting in a survey of nine to fifteen questions. Depending on the length and difficulty, the time required to administer the survey ranges from ten to fifteen minutes.

The student responses are then transformed into machine readable form and submitted to the computer for data processing. Not only are the surveys corrected, but a series of reports - to the individual student, to the course professor, and to the teaching assistant - are generated. These reports provide extensive, detailed and timely feedback to all participants in the instructional process on many facets of teaching and learning. Consider the broad categories of information on each of these separate reports. Each student receives a student report shortly after taking the survey (from 4 to 12 hours). On the report, the student is provided with a mix of assignments for the forthcoming period which, in the judgment of the instructor, is best suited to the individual student's mastery of the course materials. The assignments may vary in level, in length, in nature (repetitive, analytic, problem solving; mathematical, nonmathematical), in form (programming, problem sets), in media (television, CAI), and in the nature of the involvement with the instructor(s) (discussion sections, tutorials, special lectures). The underlying premise of TIPS, reflected in the student reports, is that the primary unit of learning is the individual, and thus instructional instruments must be directed to this level.

An equally important feature of TIPS is the feedback from students to instructors afforded through a teaching assistant report and a professor

report. These reports highlight areas of student difficulty and, in addition, provide an efficient means of coordinating many levels and types of instruction within a single class. The reports demarcate those concepts which are learned well, indicate those materials understood poorly and why (analysis of specific wrong answers), list assignments by student for the forthcoming period, and provide both the names of students having difficulty and the names of students performing particularly well. On the basis of the results, lecture content, level or pace may be altered, and new sectional assignments prepared. In essence, the TIPS system is designed to give virtually immediate information flow in both directions - a feature many consider vital to an effective educational process.

TIPS has been completely redesigned for this project's macro and micro courses, and implemented in three of the course packages, two of which have been tested in the pilot study.

1.3.2 Games

A classroom game consists of an artificial environment in which the student plays an active role in the pursuit of some objective. For the game used in one of the macro course packages, the environment is an aggregative simulation model of an economy and the student's role is that of a policy-maker trying to avoid unemployment and inflation.

An appropriately structured game has several advantages over conventional methods for teaching aggregative economic theory and policy. Students are transformed from passive to active learners; they receive continuous feedback, and have the opportunity to apply repeatedly new ideas in a variety of contexts. Since participation in a game both entices involvement, because it is fun, and requires involvement, because an active response must be made, a game situation can be expected to be relatively successful in capturing the student's attention. The response of students to teaching games bears this out. A classroom game additionally provides continuous feedback. The student learns in short order where his or her policy decisions achieve their objectives and receives immediate reinforcement in numerous and diverse concrete situations.

In the conventional classroom situation, most students do not acquire an adequate understanding of economic theory or how it relates to either economic policy or everyday economic events. In the game situation, the student is confronted by a set of data on economic variables, such as GNP, employment and inflation, and is given the objective of changing these variables through economic policy. Students are forced to use economic theory both to order and make sense of this mass of information and to instruct them on how the policy tools at their command should be used. In this context, theory will be viewed as a useful tool for understanding economic events rather than as a set of mechanical rules for manipulating abstract relationships, all of which appear unrelated to the real world.

Prior to this project, most of the sophisticated macroeconomics games have been intended for use at an advanced level. Those games which have been designed for and used at the introductory level have been seen as a form of enrichment and were essentially minor adjuncts to the

course. In the project's course packages which include games, playing of the game is an integral activity which assumes major importance to both teacher and student. A significant proportion of both class and study time is devoted to analysis and decision-making in the context of the game. Since the language and analytical techniques involved in the game playing are entirely consistent with those of lectures and readings, there is a profound interaction that takes place between the games and other teaching techniques. To date numerous components of the games to be used in the pilot study have been developed, especially for later sections of the course, but the pilot study will probably be the first time in which a truly integrated games orientated course has been taught at the introductory level.

1.3.3 Cases

The case study method is identified with Harvard's Business School where, for many courses, the case study approach represents the principal teaching technique. A case study essentially involves providing students with a written case or problem to which they must apply fundamental concepts and reach a conclusion or make a recommendation for policy action. The case may represent a real world, modified or purely hypothetical situation. Cases have appeared in economics in printed form only within the past few years. While students exposed to the case approach, and faculty incorporating cases into their economics courses tend to be enthusiastic about this technique, there is yet little hard evidence which suggests that students' economics comprehension increases significantly when cases are added to a conventional course. The principal educational advance provided by cases is that they require students to become involved in the process of analyzing relatively unstructured economic problems and synthesizing a wide range of economic concepts. Indeed, one of the common student criticisms of conventional economics courses is their failure to bridge the relationship between lectures and texts, and the events of the real world. The skills of analysis and synthesis which are essential to bridging this relationship are perhaps the most difficult to teach and yet the most important to learn. It is in this area that the case approach might have an absolute advantage.

As part of the project's activities numerous new cases have been developed to be included in the appropriate course packages. Experimentation with and evaluation of individual cases has led to substantial improvement in the case approach. A greater emphasis has been placed on the use of real world examples even though, or perhaps essentially because, this introduces complexities which are assumed away for the sake of clarity in text books, but which are unavoidable in the issues it is hoped students will be able to deal with when they finish the course.

1.4 Course Packages

As discussed above, in the context of this project, a course package is a specific combination of new and existing teaching techniques designed to teach a given body of material defined by the course outline. The new techniques were discussed in the preceding section; the existing techniques include lectures, discussion sections, textbooks, and other readings, including programmed learning. While all of these techniques

can be combined in different ways, certain combinations will be selected for implementation in the experiment of phase 2, so as to provide as much information on their effectiveness as is possible within the resource limitations of the project. In the pilot study three specific packages were tested: a macro TIPS course, a macro games course, and a micro cases plus TIPS course. Volume 2, Parts I, II and III contain the materials for these three packages plus the corresponding materials for conventional micro and macro courses and TIPS micro and TIPS cases courses. Each course package consists of a summary description of the course, a course management plan, the course outline, and materials relating to the new techniques incorporated in the package. When in final form this would constitute one of the sets of materials which would be provided to instructors in the project.

1.5 Measurement Instruments

In order to extract from the experiment as much information as possible it is necessary to devote substantial resources to the development of instruments for measuring course inputs and course outputs. These consist of both tests and questionnaires to be completed by both teachers and students. Each course package in Volume 2 contains all the necessary tests and questionnaires, many of which have common elements but most of which are unique to a given package.

1.5.1 The Economics Pre-tests

While most students participating in the experiment will not have previously taken a formal course in economics, some will have and still others will have had an exposure to the subject in general social science courses. It is desirable, therefore, in order to estimate the impact of different packages on learning, to control for differences in initial understanding of economics. This will be achieved by administering a short multiple choice test in economics at the beginning of the course. Experience has shown that even brief tests such as this can be powerful discriminators.

1.5.2 The Personality Test

An important hypothesis to be tested in the experiment is that the effectiveness of different course packages depends on the student's characteristics, both cognitive and affective. With respect to the latter category, a review of the educational psychology literature has revealed numerous instances where trait-treatment interactions have been identified. Furthermore, the questions of which dimensions of personality and which type of test might be used have been considered. Personality traits which the literature suggests might be relevant to the comparative effectiveness of the different course packages include

1. factors which facilitate achievement in a setting where conformance is a positive behavior;

2. factors which facilitate achievement in a setting where autonomy and independence are positive behaviors;

3. the degree of personal and intellectual efficiency which the individual has attained.

4. The degree of flexibility and adaptability of a person's thinking and social behavior.

While many tests are available for measuring such traits, practical implementation in the context of the experiment requires that the test can be administered in a short period of time, that it be visibly readable to participants, and it provide results which can be subjected to statistical analysis along with other data generated by the experiment. These considerations have narrowed the choice down to one of two tests: the California Psychological Inventory and the Eysenck Personality Inventory. For the purposes of the pilot study, four components of the CPI were administered to students at the beginning of the course.

1.5.3 Student Questionnaires

In addition to taking an economics pre-test and the personality test, students will be asked to complete a factual questionnaire. The questionnaire will provide information on students' educational and socio-economic background, their attitudes toward their present course of study, and their educational and career objectives. Additionally students will be asked to complete questionnaires during and at the completion of the course on the content of the course, the contribution of the different teaching techniques to their comprehension, their overall evaluation of the course, their opinion and enjoyment of the various techniques, and their allocation of time.

1.5.4 Teacher and Course Questionnaires

The instructors who participate in the experiment will also be asked to complete questionnaires relating to their own characteristics and those of the course and institution. Teacher characteristics of interest include educational background, teaching experience, and time spent on teaching and other professional activities, such as research and administration. With respect to course characteristics, instructors will be asked to provide information on variables such as class size, course pre-requisites, time of day when lectures are given, background of teaching assistants, etc. Information will also be obtained from instructors about individual student characteristics, such as SAT scores, grade point average, etc., which is available from school records. Instructors will be asked also to evaluate each teaching technique and state adoption plans.

1.5.5 Economics Post-test

The most critical measuring instrument is the post-test which will provide data on the effect of all the input variables, including which course package is used. The problem of measuring educational output is extremely complex and will require a fairly sophisticated test instrument. In most large scale educational experimentation, heavy reliance has been placed on objective tests because of the ease of

grading and interpretation. It is generally agreed, however, that learning is a multi-dimensional phenomenon which cannot adequately be measured by the standard objective tests. In terms of the Bloomian taxonomy, learning involves more than passive understanding of the material covered in the course. It also includes the acquisition of the ability to apply, to analyze, to synthesize, and to evaluate economics concepts and phenomena.

In order to measure these components of educational output, the students participating in the experiment will be given an examination for credit at the end of the course composed of the following elements:

1. objective test questions which measure recognition and understanding of basic concepts as well as simple and complex applications of concepts.

2. essay questions which measure complex applications and analysis of well defined problems.

3. case analyses of unstructured problems which measure the ability to identify problems, select the relevant economic principles, apply these principles and recommend appropriate policy.

1.6 The Pilot Study

The objective of the pilot study is threefold. First, it will be used as a means of working out the logistics to be employed in the experiment. Inevitably, numerous problems relating to timing and organization will not become apparent until the new course packages are utilized in experimental conditions. Based on the experience of the pilot study a set of procedures will be established to ensure that the large scale experiment can be carried out free of unanticipated crises.

Second, the process of using the new course packages and measuring instruments will produce information about how the materials themselves can be improved. This information will be used to revise the materials prior to their use in the experiment.

Third, the experience of the pilot study will inevitably yield implications for the ways in which the final course packages, scheduled for construction in phase two, should be designed, and will provide information on training necessary for instructors who will be involved in the experiment. The findings of the part of the pilot study completed to date are contained in section 2 in this volume.

1.7 The Model and Research Design

Underlying and motivating the design of the various measuring instruments to be utilized in the experiment is a model of the educational process which relates outputs to inputs. Formally, the model defines the variables to be included in the analysis and which, therefore, have to be measured, and specifies the functional form of the relationships between variables. Given the model, the problem becomes one of obtaining estimates of the model's parameters from the data generated by the experiment. In order to make sure that such inferences can be

made reliably once the experiment is completed, it is essential that considerable thought be given ahead of time to the statistical design of the experiment.

During Phase 1, a considerable amount of refinement of the model has occurred. Numerous decisions about which variables should be included in the analysis have been made and these are reflected in the measuring instruments contained in the course packages. Furthermore, a good deal of thought has been given to theoretical considerations from both the educational literature relating to the psychology of learning and the economics literature relating to functional structure which bear on the form of the input-output relationships. In this connection, while the literature is suggestive, it is not sufficiently well developed to provide the project with a generally accepted theory of education production, except in very general terms. Consequently, it has become one of the tasks of the project to construct such a theory and embody it in the statistical model to be estimated from experimental data.

With respect to the problem of experiment design, the work completed thus far has been largely devoted to meshing the classical notions of optimal design with regression related statistical methods and to forming preliminary judgments about the relationship between the number of course packages to be evaluated and the number of institutions and treatments per institution which must be included in the experiment to produce reliable results. The main conclusion reached in this connection is that, for the size of the experiment discussed in the original proposal, it may not be possible to evaluate meaningfully more than eight packages, four in micro and four in macro. These topics are dealt with at length in Section 3 in this volume.

2 PILOT STUDY

2.1 Introduction

The three institutions which participated in the pilot study were the 'home' universities of the three principal investigators. This proximity permitted direct observation of logistics problems as they arose and provided on hand experience to deal with them. Since there were no control groups no meaningful comparisons are possible between the effects of the new course packages and conventional counterparts. However, preliminary analyses of the data collected provide results which are indicative of the potential of the new course packages to increase efficiency significantly in higher education. In addition these data yield tentative findings regarding the reactions of students to the new teaching techniques.

2.2 Pilot Institutions

The Heriot-Watt study involved 280 students in the micro portion of the principles course which ran from October 1977 to February 1978. The new techniques incorporated in the package were TIPS and Cases. A complete description of the core of the course covered in the first ten weeks of the semester is contained in the TIPS-Cases Course Management Plan in Volume 2. The remaining four weeks of the course were devoted to topics of special interest including a significant portion of institutional material on the British economy.

The Duke University study involved 180 students in the macro portion of the pinciples course which ran from September to December 1977. The new technique incorporated in the course package was TIPS and a complete description of the course is also contained in Volume 2.

The University of California at San Diego study is ongoing and will terminate in April 1978. The course began in January 1978 and contains 110 students; it incorporates a macro simulation game for use on time sharing computers, accompanying research assignments, and intensive use of a programmed text in the first two weeks of the quarter.

The pilot studies have proved to be invaluable in highlighting logistics problems, isolating course inputs which require revision and, in one of the institutions, leading to a complete reorganisation of the principles course. The logistics problems encountered centred mainly on the resources required, especially the time of all concerned, simply to distribute materials and collect course outputs; in addition, in the Micro-TIPS-Cases course a proportion of students were unable to reproduce accurately their identification numbers on request which led to lags in the transmission of information to students and instructors because of the need to re-run in the computer the missing observations. However, no serious problems associated with the heavy computer use were encountered, though this may reflect the careful preparation and planning which took place between the instructors and their respective computer centres.

Most of the materials associated with the new techniques proved successful, as did the questionnaires and measuring instruments. Significant revision is planned for the macro course units as the content of the macro course with a mathematical modelling bias proved difficult for a significant fraction of students. Some revision of the micro units and reallocation of time by topic is planned also. The revisions of all the course inputs will reflect not only the information generated in the pilot study but also the opinions of academics reviewing the materials.

Finally, the pilot study has provided data on the type of result which is predicted to emerge in the full experiment. The preliminary findings relating to student learning, student opinion and resource reallocation within teaching strongly indicate that the full scale experiment will generate data of fundamental importance to efficiency in economics teaching. Indeed, the results indicate that there is a potential for a virtual revolution in the methods of teaching economics and that there are valuable lessons to be learned for efficient resource use in the teaching of other subjects.

2.3 The Heriot-Watt Course

This course package resulted in the most dramatic change in course organisation ever seen in a UK university. The conventional and well tried method of tutorial instruction was abandoned in favour of a weekly TIPS and a weekly Cases remedial tutorial, supplemented by five office hours per week on the part of the course co-ordinator and the professor. Normally, the class of 280 students would have been split into 28 tutorial groups per week, with a concomitant heavy input of faculty time. The data generated on student opinion, the use of the techniques by students, student learning and the costs of the course show that it is possible to completely alter the cost effectiveness of teaching economic principles to large classes.

1 Student Opinion. Students were asked to rate the course on a scale from 1 to 5 in which low = 1 and high = 5, and its main characteristics on a scale from 1 to 5 in which 'strongly disagree' = 1 and 'strongly agree' = 5. The average ratings are set out in Table 3.2(1).

Table/

Table 2.3(1) The Heriot-Watt Course: Student Ratings

The course contributed considerably to my understanding of economics	4.2
The course was interesting	4.1
The course was too advanced	2.4
The course put undue emphasis on theory	2.8
The course helped in the understanding of real world situations	3.7
The course stimulated me to further study	3.0
The course involved a high volume of written work	2.8
The course involved a high volume of reading	3.7
The recommended reading was well integrated with the lecture material	3.9
The sequence of topics covered was logical	3.9
The sequence of topics covered helped understanding	3.9

On the scale below circle the number which reflects your overall rating of this course:

	LOW				HIGH	
Course rating	1	2	3	4	5	3.9

Some care is necessary in considering the ratings of the characteristics since in some cases a high rating does not mean a high degree of satisfaction. While the rating of the course contribution to understanding was 4.2, implying that students felt they had benefited considerably from the course, the rating for the course being too advanced was 2.4, implying that on average the standard was just about right. The course rating of 3.9 was the highest rating of large undergraduate courses in the university.

In addition to their opinion of the course students were asked for their assessment of the contribution which each technique had made to their understanding. Table 2.3(2) sets out the question asked and the distribution of learning among the techniques.

Table 2.3(2) The Heriot-Watt Course: Contribution to Learning by Technique

Allocate 100 points among the teaching methods to reflect the contribution each has made to your understanding of this course.

Lectures	Text-book	Prog. Text	Read-ings	TIPS	Cases	Tutorials TIPS	Tutorials Cases	None of These	TOTAL
39	11	17	3	12	12	2	1	3	100

While students feel that lectures make the biggest single contribution to learning, TIPS and Cases between them accounted for 24%. In the Heriot-Watt course the tutorials were remedial in the sense that while open to all students, those students performing poorly in TIPS and/or Cases were strongly advised to attend. The small apparent contribution of the tutorial reflects the fact that the bulk of the class did not use this particular technique. For regular tutorial attendees, i.e, the poorer students, the contribution of tutorials was 12. The fall experiment will provide data which will show the changes in perceived student learning as a result of substituting the TIPS-Cases course for example for the conventional tutorial/lecture course in addition to actual changes in learning.

2 Use of Techniques. With very few exceptions the students were eager to obtain the feedback information provided by the TIPS outputs and the graded case studies and model answers. About 90% of the Reports and graded Cases were collected within a few hours of being returned to the collection point. Although the TIPS Surveys and Cases were voluntary they were completed by a high proportion of students, although as might be expected the numbers declined slightly as the term progressed and term examinations in other subjects neared. After the fourth TIPS Survey the students were allowed to take the surveys home and complete them overnight; this was in response to complaints by some students that they did not really have time in the ten minutes allotted in class to do justice to their understanding. It appears that this led to a reduction of about 8% in the number of completed Surveys handed in. On average, students completed 7.6 out of the 9 Surveys (84%), and 4.7 out of the 8 Cases (59%). Of the 17 voluntary assignments, the minimum number completed was 4.

An important area of the final research will consider the effect of substituting one technique for another at the margin. For example, do students learn more from doing an additional case or an additional TIPS survey? While the answer to this question must await the full experimental phase of the research, a multiple regression was run on the Heriot-Watt data to 'explain' performance on the final exam. The control variables used were initial understanding (pre TEC score) and IQ (AH5 score), while the 'technique' variables used were number of Cases done, number of TIPS done, and the fact of having done the 'mock final' exam. The regression statistics are set out in Table 2.3(3).

Table 2.3(3). Student Learning and Use of the Techniques

Dep.Var	Independent Variables						
Final Mark	No. Cases	No. TIPS	AH5 (1)	AH5 (2)	Pre TEC	no Pre TEC	Mock Final taken
(%)	2.6	-0.3	0.5	-0.3	0.9	3.6	0.9
	(6.7)	(0.6)	(2.6)	(1.5)	(2.6)	(1.2)	(0.4)

R^2 .30: Constant 40.9: F ratio 13.4

Each additional Case done was associated with an additional 2.6% on the final examination, while the number of TIPS done did not seem to matter. It should be noted that this can be interpreted in two ways. First, it may be that the variation in the number of TIPS Surveys done was not great enough to produce statistical findings (the standard deviation divided by the mean for the number of TIPS done was .25 compared to .49 for number of Cases done). Second, it may be that the fact of doing Cases adds to the student's mastery of the skills tested by the final exam. The full experiment will provide the data and results needed for the optimal design of courses for different types of students.

3 Student Learning. Without appropriate controls it is not possible to compare learning in the new courses with learning in a conventional course. However, in the objective part of the final examination one-third of the questions were identical to, or matched closely, to questions set in the previous year's principles course. These questions were not available to students for study purposes prior to the final examination this year as the examination paper for the previous year had been withdrawn. While detailed student characteristics for both years have yet to be compared, there is no reason to expect significant differences with respect to age, sex mix, grade point average, IQ, or class composition by major.

In 1976 - 77, when 220 students attended the course, the percentage correct on the common questions was 61%; the matching figure for the 280 students attending the 1977 - 78 course was 69%, giving a gain of about 13%. Since this was not a controlled experiment these findings can only be regarded as tentative, but taken together with the subjective evaluations of the course they are indicative of the potential of the new techniques for increasing efficiency in the teaching/learning sphere.

4 Course Costs. Based on Heriot-Watt experience the cost of the conventional ten week micro course can be compared to the cost of the TIPS-Cases micro course. The duration of the complete micro course is fourteen weeks, ten of which are devoted to the core material contained in the micro units. To provide a basis of comparison the weekly costs of both types of course are estimated from empirical data.

Table 2.3(4) contains data for both courses.

Table/

Table 2.3(4). Course Inputs

Inputs Number per week	Conventional Course	TIPS/Cases Course	Hourly Wage/ Price per Unit
Lecture Hours	L	L	W_1
Tutorial Hours	$\alpha_1 S$	α_2	W_1
Course Co-ordinator Hours	α_3	α_3	W_1
Printed Pages	$\alpha_4 S$	$\alpha_5 S$	P_1
Grading Hours	$\alpha_6 S$	$\alpha_7 S$	W_1
Course Assistant Hours	-	$\alpha_8 S$	W_2
Card Punch Hours	-	$\alpha_9 S$	W_2
CPU Hours	-	$\alpha_{10} S$	P_2
Computer Print-Out Pages	-	$\alpha_{11} S$	P_3

From the data in Table 2.3(4) the cost functions were constructed

Conventional Course (costs per week)

$$TC = LW_1 + \alpha_3 W_1 + S(\alpha_1 W_1 + \alpha_4 P_1 + \alpha_6 W_1)$$

$$MC = \alpha_1 W_1 + \alpha_4 P_1 + \alpha_6 W_1$$

TIPS-Cases Course (costs per week)

$$TC = LW_1 + \alpha_3 W_1 + \alpha_2 W_1 + S(\alpha_5 P_1 + \alpha_7 W_1 + \alpha_8 W_2) + S(\alpha_9 W_2 + \alpha_{10} P_2 + \alpha_{11} P_3)^*$$

$$MC = \alpha_5 P_1 + \alpha_7 W_1 + \alpha_8 W_2 + (\alpha_9 W_2 + \alpha_{10} P_2 + \alpha_{11} P_3)^*$$

---

* costs to the computer centre

Table 2.3(5) contains cost data for each course input and total and marginal costs for each course using the above functions. The parameter values and assumptions are contained in the footnote below.

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1 The length of the course is 10 weeks. To calculate total course costs weekly costs are multiplied by 10.

2 L = 3; 3 lectures per week of 50 minutes duration are common to both courses.

3 S = 200; the number of students in the class is assumed to be the same in both courses.

4 $\alpha_1 = \frac{1}{\text{tutorial size}}$; for 200 students and an average tutorial size of 10 students, i.e., $\alpha_1 = \frac{1}{10}$, the number of tutorial hours per week is 20 and for the course, 200.

5 $\alpha_2 = 2$; 1 remedial TIPS tutorial and 1 remedial case tutorial.

6 $\alpha_3 = 1$; the course co-ordinator spends, on average, 1 hour per week.

7 $\alpha_4 = 1.5$; 15 printed pages of course syllabus and essays are distributed in 10 weeks.

8 $\alpha_5 = 8$; 80 printed pages of course syllabus, cases and TIPS surveys are distributed in 10 weeks,

9 $\alpha_6 = .025$; 3 essays per term each consuming 5 minutes of grading time.

10 $\alpha_7 = .05$; one case is assigned each week. The 3 minutes grading time per registered student is empirically based. While each case, on average, consumes more than 3 minutes to grade, not all students complete each case.

11 $\alpha_8 = .05$; 3 minutes clerking time per student per week is empirically based. The course assistant is responsible for collating and preparing TIPS surveys for card punching, and distributing computer print-outs and cases.

12 $\alpha_9 = .004$; card punching for 200 registered students doing TIPS requires approximately 48 minutes per week.

13 $\alpha_{10} = .001$; computer processing time for an average TIPS survey requires approximately 12 minutes. 1 TIPS survey is given each week.

14 $\alpha_{11} = 3.0$; a TIPS survey output comprising student, tutor, and professor reports for 200 students requires approximately 600 pages of computer print-out.

15 W_1 = £20; assuming an average salary cost of £6,000 per annum for lecturer, tutors and graders and 300 lecturing, tutoring, grading hours per year.

16 W_2 = £2; hourly wage of course assistant.

17 P_1 = £.015; 1.5 pence per printed page.

18 P_2 = £30; £30 per hour of CPU time.

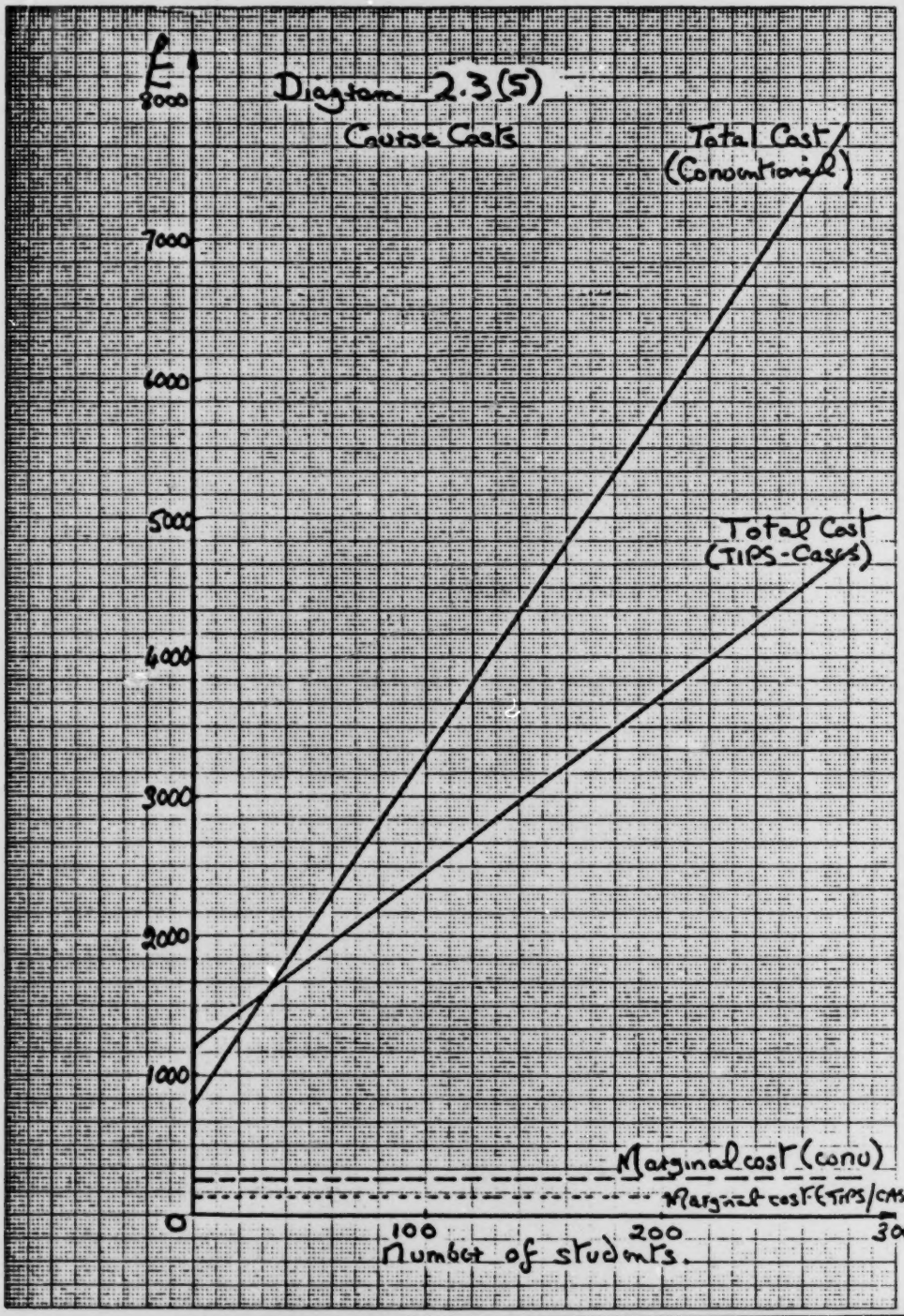
19 P_3 = £.005; .5 pence per sheet of computer print-out.

Table 2.3(5). Course Costs

	Lectures	Tutorials	Course Co-Ordinator	Essay/Case Grading	Card Punching	TIPS Clerking	CPU	Paper Regular	Paper Computer	TOTAL
Conventional Course										
Total Cost	600	4,000	200	1,000	0	0	0	45.00	0	5,845.00
Marginal Cost	0	20	0	5	0	0	0	0.23	0	25.23
TIPS/Cases Course										
Total Cost	600	400	200	2,000	16.00	200	60	240.00	30.00	3,746.00
Marginal Cost	0	0	0	10	00.08	1	00.3	1.20	00.15	12.73

These data, graphed in Diagram 2.3(5), indicate that for a 200 student course the total cost is reduced by about one-third (36%) and the marginal cost is halved by adopting the TIPS/Cases course. Slight variations in the assumptions, such as varying tutorial size, and changing the number of lecturers per week or essays per week, will affect the relative total costs as will the numbers of students. The costs for the TIPS/Cases course could be substantially reduced by using objective questions in certain cases instead of requesting written reports and computer processing the answers in a TIPS format. To the extent that one half of the cases could be tackled in this fashion, grading costs could be halved, a saving of £1,000, with only a marginal increase in computer costs, approximately £150.

Total costs per student are equal for both courses for approximately 32 students. For small numbers the conventional course is cheaper per student, the gain of the TIPS/Cases course increasing with student numbers.



2.4 The Duke Course: Opinion of Course, Use of Techniques and Implications for Package Design

The use of TIPS in the macro course did not involve the fundamental change in resource allocation described in the Heriot-Watt course, since the students were provided with a weekly tutorial (the content of which was influenced by the class performance on TIPS Surveys); in addition a TIPS Debriefing Session was held each week which concentrated on the questions with which students had had difficulty. A slightly modified use of TIPS was made in that the students did not retain their copies of the TIPS Surveys (in the interests of maintaining the confidentiality of the questions for the experimental stage), and students were provided with a detailed lecture outline prior to the lecture. At the Debriefing Session the questions discussed were displayed on a screen.

1 Student Opinion of Course. The evaluation form did not utilise rankings for the detailed aspects of the techniques, but relied on an open-ended format backed up by oral guidelines; this provided the opportunity to compare the type of information gained by the more detailed type of questionnaire set in the Heriot-Watt course which allows students less scope for expression. The comments refer to the course content, the TIPS Surveys, the examinations and the course goals. While valuable information was generated by this approach, particularly with respect to future course planning, in the final experiment student opinions will be collected in a form suitable for statistical analysis.

The majority of students considered the course to be quite difficult and relatively mathematical in orientation; although Duke Freshmen are above average academically and tend to have a mathematical background, a significant number felt the mathematical approach constraining. Two distinct groups emerged on this issue, with two-thirds of the class expressing comfort with the level of mathematics, and one-third expressing the view that it hindered their learning. The detailed lecture notes were considered helpful and the outline materials indispensable to mastering the course objectives. While the course was considered difficult, those students who mastered it felt satisfied with their learning experience, and a larger cohort than usual comitted themselves to majoring in economics. On the other hand, a larger cohort than normal stated that they were no longer interested in economics as a major, leaving a very small undecided category. This bimodal polerisation of student evaluations and decisions was a significant feature of the course. Overall, the course was well received, ranking second out of the 28 undergraduate courses in the economics department. In this respect, students were asked to rate each course 'bad', 'poor', 'fair, 'good', 'excellent', and these were transformed into a numerical scale ranging from 1 to 5. The rating for the course for 180 students was 4.24.

This high rating for the course was achieved in spite of the perceived difficulty of the course and the fact that students felt they had spent more time working on the course than other subjects.

Students felt that the TIPS Surveys accurately reflected the course objectives, and gave a good indication of the scope and level of the course examinations.

2 Use of Techniques. The voluntary participation in the TIPS Surveys averaged 96%, which was also a reflection of the high attendance at the lectures. Average student performance on the TIPS Surveys was about 80%, indicating that students were coping well with the course, despite the opinion that it was on the whole difficult. The Debriefing Sessions were well attended and students considered them of value to their performance by the end of the course.

3 Implications for Package Design. This was the first test of the content of the Macro Core Units and it is clear that for the average students less emphasis will have to be placed on the mathematical exposition. This is based on the fact that Duke students are not representative of all university beginning economics students and if a significant proportion experienced difficulty with the mathematical approach this will be even more pronounced in the majority of other universities.

Some aspects of the Units will have to be substantially revised. For example students are not particularly happy with the concepts of the Phillips Curve and Okun's Law because of the lack of predictive ability. However, part of the next stage of the project will involve the revision of the macro units in the light of comments from academics and teachers.

The general lessons for the Duke Pilot are, first, that while the TIPS macro package is well suited to overcoming the difficulties with the basic ideas of macroeconomics, there is no doubt that the other packages based on the macro simulations will have more potential in areas of policy, and in dealing with real life applications of the ideas. Second, the mere adding on of TIPS to a conventional course based on lectures and tutorials, while providing no savings in resources, does generate information for both staff and students which is considered to be helpful by both parties. However, the precise impact of the add-on can not be detemined without recourse to the type of data which can be generated in the full scale experiment.

2.5 The University of California at San Diego Course

This course is still in progress and hence no objective output data are available. However, at this stage a number of impressions can be reported. This course utilises the macro simulation games and subjects students to two weeks of intensive study of the programmed texts with the objective of identifying those with potential problems early on in the course. Those students having difficulty with the texts have been provided with additional remedial tutorials and office hours.

The computer simulations and the accompanying assignments have elicited excitement and interest on the part of the students, and the five teaching assistants report that students' reactions are positive. Many students, who study in groups in preparation for running the economy, have commented on how much they feel they have learned from discussions with their peers.

No logistics problems have been encountered with the relatively large class sizes involved in this type of course, and the way is now set for using the simulations in classes of even larger sizes, given that the hardware is available in the university.

3 RESEARCH DESIGN

3.1 Introduction

In this section, we review briefly the classical principles of experimental design, indicate some limitations with these concepts when applied to the problem of designing a large scale educational experiment, and delineate the fundamental issues involved in designing such an experiment.

We emphasize the importance of the overall objective of the experiment, and show that the experimental objectives cannot be specified independently of the experimental environment. For instance, a crucial aspect of the overall problem is whether a single treatment is to be chosen which will then be applied to all subjects. Where treatment effects and the effects of characteristics of the subjects are not independent of each other, it is not necessarily optimal that a single treatment should be chosen. Instead, it may be that different subjects should be given different treatments, depending on their observable characteristics. Thus the objective of the experiment should not necessarily be to find the "best" treatment.

The discussion delineates the experimental design issues which arise in different experimental environments. In the educational context, it is invalid to treat the experimental design as simply relating to a single interpretation of the classic design concepts. Instead, one must note that a series of interpretations exist, in which the "plot/treatment" dichotomy is translated into student/course, faculty/course, and course/university. This situation, in which experimental design issues exist at several different levels, renders simple analyses at any single level invalid. By surveying the classic experimental design concepts, we attempt here to show the several levels of analyses which are needed.

The difficulties - and in some dimensions, the impossibility - of rigorously applying strict experimental design principles are also indicated. Since these principles cannot be applied perfectly in this type of educational experiment, it is important to consider the rationale for the classic experimental design techniques and the nature of inevitable discrepancies. Proponents of strictly controlled experiments sometimes appear to conclude that unless all of the theoretical criteria can be satisfied, statistical inferences are worthless. That extreme position is clearly unwarranted. Indeed, limiting research to those questions for which completely controlled experiments can be developed means that only a small proportion of policy issues could be addressed by empirical research. The more realistic procedure is to carefully analyze the discrepancies between the feasible experiment and the theoretical ideal so that the nature of the implied problems can be understood and so that the efficacy of compensating adjustments in the experimental design can be evaluated.

As noted in the proposal, the Fisherian experimental design paradigm uses two basic techniques to purge experimental inferences of systematic error. These are randomization and orthogonalization. Where either randomization or orthogonalization is not perfectly

attainable, compensating adjustments in the statistical procedure can be made using "covariates", a term which refers to supplementary observable variables, and which will become clearer below. Let us begin the discussion by indicating how covariates can be used to partially mitigate imperfect randomization. We begin with the classic "fertilizer" experiment, and then draw on the obvious analogy between fertilizer and education.

3.2 The Classic Experimental Design Paradigm

Suppose that four types of fertilizer are to be tested for their effect on the yield of a particular crop. Assume that five plots are available. Then the yield of each plot would depend on three sets of factors. First, the relevant characteristics of each plot which are intrinsically associated with the plot, such as the mineral content of each plot and perhaps the underground drainage properties of the plot. Second, there are a number of factors which affect the yields and vary from experiment to experiment in a haphazard and totally unpredictable manner, such as the quality of individual seeds and the vagaries of pollinating birds. Third are the effects of the different fertilizers.

Suppose first that the five plots are each planted without any fertilizer. Then only the first set of factors, the "systematic effects of the plots", and the second set, the "haphazard factors", are present. Thus we can write the ultimate yield of the j^{th} plot, Y_j, as equal to a function of the two sets of variables:

$$Y_j = F(M_j, D_j, \ldots ; e_j^1, e_j^2, \ldots)$$

where M_j represents mineral content, D_j represents drainage properties, and so forth, and the e_j represent the haphazard "random" factors which are unique to the individual experiment and essentially unpredictable.

If we agree to conceptualize the e_j as random variables with a well defined probability distribution, then Y_j will have a mean over repeated experiments:

$$E(Y_j) = E\left[F(M_j, D_j, \ldots ; e_j^1, e_j^2, \ldots)\right] = \gamma_j$$

which we have denoted as γ_j. This statistical expectation is taken over the joint probability distribution of all of the e_j^1. In other words, if the experiment is repeated on the j^{th} plot many times, the average yield on the j^{th} plot should average out to be γ_j, with deviations accounted for by the set of haphazard factors. We can then formally define Y_j as the sum of a systematic mean, γ_j, and the deviation from that mean on the particular experiment in question, which we call ε_j. In other words we can write:

$$Y_j = \gamma_j + \varepsilon_j$$

We can call deviation in the γ_j the "plot effects" and the ε_j the "random effects". Now as noted, one way to obtain reliable estimates of the plot effects would be to repeat the experiment a large number of times on each plot; labelling these repetitions by time, one would have:

$$Y_j^t = \gamma_j + \varepsilon_j^t \qquad t = 1,T$$

and the average yield for each plot, $\bar{Y}$, would then be:

$$\bar{Y}_j = \frac{1}{T}\left(\sum_{t=1}^{T} Y_j^t\right) = \gamma_j + \frac{1}{T}\left(\sum_{t=1}^{T} \varepsilon_j^t\right)$$

Using very weak assumptions about the joint probability distribution of the ε_j^t, the average random effect will become negligible for large T and converge to zero with certainty.

It is useful to contrast this method for estimating the plot effects with the different approach, in which more than one plant is <u>simultaneously</u> grown on each plot.

That is, suppose that N_j plants are each grown on the j^{th} plot during the same time period. Letting i index the different plants, we can write:

$$Y_{ij} = \gamma_j + \varepsilon_{ij} \qquad i = 1, N_j$$

where Y_{ij} is the yield of the i^{th} plant in the j^{th} plot. Just as in the situation above, we can write the average yield in terms of the plot effect and the average random effect, were we now use $\hat{\gamma}_j$ for the average:

$$\hat{\gamma}_j = \frac{1}{T}\left(\sum_{i=1}^{N_j} Y_{ij}\right) = \gamma_j + \frac{1}{T}\left(\sum_{i=1}^{N_j} \varepsilon_{ij}\right)$$

However, whereas it seems quite reasonable to assume that the ε_j^t are statistically independent for different time periods, there are many reasons for expecting some of the same haphazard effects to affect all of the plants in the same plot when they are grown simultaneously. Thus a systematic element - for instance, the amount of rainfall on all of the plots - may be present, and the average random effect:

$$\frac{1}{T}\left(\sum_{i=1}^{N_j} \varepsilon_{ij}\right)$$

may not be expected to converge to zero even if N_j is increased indefinitely. Nevertheless, it is still true that there is no bias in the procedure, since:

$$E(\hat{\gamma}_j) = \gamma_j$$

even though one may not be assured that $\hat{\gamma}_j$ will necessarily converge to γ_j even as the number of plants becomes infinitely large.

3.3 The Experimental Environment: A General Specification

Now consider the actual purpose of the experimentation, namely to evaluate the effects of fertilizer on yields. In order to examine the effects of fertilizer, we must generalize the nature of the function defining the determination of yield. Let us denote the

amounts applied of the four fertilizers as F_2, F_3, F_4, and F_5, for notational convenience. In general, we could then write the yield for a plant on the j^{th} plot as:

$$Y_j = G(F_2,F_3,F_4,F_5;\ M_j,D_j,\ldots;e_j^i,e_j^i,\ldots)$$

so that the relationship of the functions F and G is:

$$F(M_j,D_j,\ldots;e_j^1,e_j^2,\ldots) = G(0,0,0,0;M_j,D_j,\ldots;e_j^1,e_j^2,\ldots)$$

3.4 The Purpose of Randomization

Let us then assume that fixed amounts F_j^* of each fertilizer are separately added, in sequence, to the same numbered plots, so that:

$$Y_1 = G(0,0,0,0;M_1,D_1,\ldots;e_1^1,\ e_1^2,\ldots)$$

$$Y_2 = G(F_2^*,0,0,0;M_2,D_2,\ldots;e_2^1,e_2^2,\ldots)$$

$$Y_3 = G(0,F_3^*,0,0;M_3,D_3,\ldots;e_3^1,e_3^2,\ldots)$$

$$Y_4 = G(0,0,F_4^*,0;M_4,D_4,\ldots;e_4^1,e_4^2,\ldots)$$

$$Y_5 = G(0,0,0,F_5^*;M_5,D_5,\ldots;e_5^1,e_5^2,\ldots)$$

Now we can take expectations over the joint probability distribution of the e_j terms, just as above, to obtain:

$$E(Y_1) = \gamma_1$$

$$E(Y_2) = \gamma_2$$

$$E(Y_3) = \gamma_3$$

$$E(Y_4) = \gamma_4$$

$$E(Y_5) = \gamma_5$$

where γ_j is chosen to denote the expected yield. Now suppose that the researcher accepts

$$(Y_j - Y_1) \qquad\qquad j = 2,5$$

as an estimate of the effect of the j^{th} fertilizer.

Clearly this is not a valid estimate, since its expected value would not be zero had there been no fertilizer applied. In our notation, the expected value for $(Y_j - Y_1)$ is equal to:

$$\gamma_j - \gamma_1$$

in the absence of fertilizer, and equal to

$$\rho_j - \gamma_1$$

in the presence of fertilizer. The net effect of adding the j^{th} fertilizer to the j^{th} plot, in terms of the change in expected yield, is actually:

$$(\rho_j - \gamma_j)$$

By using $(Y_j - Y_1)$ to measure the effect of adding the j^{th} fertilizer, one has added a systematic bias to the measured effect, since:

$$E(Y_j - Y_1) = \rho_j - \gamma_1$$
$$= (\rho_j - \gamma_j) + (\gamma_j - \gamma_1)$$

We have described the basic situation which represents the fundamental rationale for randomization, as developed by R.A. Fisher. Where treatments are applied to subjects (a "subject" in this context is a specific plant located in a specific plot) in some arbitrary way, there is no protection against the possibility that systematic influences may impart biases into the analysis which would not be eliminated by repeating the experiment or by adding more subjects.

Fisher's answer was to insist that treatments be applied randomly to subjects, and that great care be taken to ensure that the actual device used to assign treatments conform as closely as possible to the theoretical ideal. By randomizing, systematic biases can be avoided.

3.5 The Limits of Randomization

But the problem is not as straightforward as this brief discussion may suggest. Let us consider the application of randomization to the present context.

Suppose that the four types of fertilizer are randomly assigned to the five plots, with one plot left with no fertilizer. In other words, one of the 120 permutations of five objects is chosen, and then the "control" and the four fertilizers are applied in the order randomly selected. Let us write this permutation as $(I_1, I_2, I_3, I_4, I_5)$. In other words, if $I_1=4$, $I_2=1$, $I_3=2$, $I_4=3$ and $I_5=5$, then no fertilizer is applied to the fourth plot, F_2^* is applied to the first plot, F_3^* is applied to the second plot, and so forth.

According to our very general assumption about the effects of the different fertilizers, there are five possible outcomes for the yield on each plot in terms of the expected outcome. Let us write $E_j(i)$ for the expected yield of a plant in the j^{th} plot to which the i^{th} treatment is applied. In other words, for the third plot we have the

five possibilities:

$$E_3(1) = E\{G(0,0,0,0;M_3,D_3,\ldots;e_1^3,e_2^3,\ldots)\}$$

$$E_3(2) = E\{G(F_2^*,0,0,0;M_3,D_3,\ldots;e_1^3,e_2^3,\ldots)\}$$

$$E_3(3) = E\{G(0,F_3^*,0,0;M_3,D_3,\ldots;e_1^3,e_2^3,\ldots)\}$$

$$E_3(4) = E\{G(0,0,F_4^*,0;M_3,D_3,\ldots;e_1^3,e_2^3,\ldots)\}$$

$$E_3(5) = E\{G(0,0,0,F_5^*;M_3,D_3,\ldots;e_1^3,e_2^3,\ldots)\}$$

where the expectation is taken over the joint distribution of e_1^3, e_2^3, and the rest of the haphazard influences. Thus in terms of our previous notation we have:

$$E_j(1) = \gamma_j \qquad j = 1,5$$

$$E_j(j) = \phi_j \qquad j = 2,5$$

while $E_2(4)$ refers to the expected yield when F_4^* is applied to plot 2, and so forth.

Now consider the inferred effect of the different fertilizers. The difference between the plot with the second treatment and the control plot is given by:

$$Y_{I_2} - Y_{I_1}$$

and similarly the inferred effects of F_3^*, F_4^* and F_5^* are given by:

$$Y_{I_3} - Y_{I_1}$$

$$Y_{I_4} - Y_{I_1}$$

$$Y_{I_5} - Y_{I_1}$$

respectively.

3.6 Inferring Average Treatment Differentials

Focussing on the inferred effect of F_2^*, the question becomes: what is it that $(Y_{I_2} - Y_{I_2})$ estimates? Randomization has been applied to eliminate bias; presumably by randomizing, we now have an unbiased estimate of something. However, it is useful to consider just what this conceptual "effect of fertilizer two", for which we now have an unbiased estimate, corresponds to.

There are twenty possible ordered pairs of the five plots to which I_1

and I_2 can correspond. We can thus write the expectation for $(Y_{I_2} - Y_{I_1})$, where we now refer to the expectation for this difference as computed before the permutation $(I_1, I_2, I_3, I_4, I_5)$ is chosen. In other words, the expectation is taken over the joint probability distribution of both the haphazard factors which apply to each plot during the experiment (the e_1^j, e_2^j,...) and the random choice of treatments.

Then we can write:

$$E(Y_{I_2} - Y_{I_1}) = \frac{1}{20}\left[\Sigma_{I_2=1}^{5}\left[\Sigma_{I_1\neq I_2} E_{I_2}(2) - E_{I_1}(1)\right]\right]$$

$$= \frac{1}{20}\Sigma_{I_2=1}^{5}\left[\Sigma_{I_2\neq I_2} E_{I_2}(2)\right] - \frac{1}{20}\Sigma_{I_2=1}^{5}\left[\Sigma_{I_1\neq I_2} E_{I_1}(1)\right]$$

$$= \frac{1}{20}\Sigma_{I_2=1}^{5}\left[4E_{I_2}(2)\right] - \frac{1}{20}\Sigma_{I_1=1}^{5}\left[4E_{I_1}(1)\right]$$

$$= \frac{1}{5}\left[\Sigma_{j=1}^{5} E_j(2)\right] - \frac{1}{5}\left[\Sigma_{j=1}^{5} E_j(1)\right]$$

In other words, the "treatment effect" which is implicitly estimated is the average effect over the five plots. If we define:

$$A_k = \frac{1}{5}\left[\Sigma_{j=1}^{5} E_j(k)\right] \qquad k = 1,5$$

the A_k represents the average expected yield over the five plots for each of the five treatments. Then the estimated treatment effect for fertilizer two is an unbiased estimate in the sense that:

$$E(Y_{I_2} - Y_{I_1}) = A_2 - A_1$$

However, it is important to note that this estimate is unbiased only in the *ex ante* sense; that is, it is an unbiased estimate taking both the random treatment assignment and the haphazard effects into account. But the expectation of $(Y_{I_2} - Y_{I_1})$ given that the plots have been selected, is not $(A_2 - A_1)$. Given that plots have been selected, the *ex post* expectation of $(Y_{I_2} - Y_{I_1})$ is:

$$E_{I_2}(2) - E_{I_1}(1)$$

That is, randomization does not eliminate the systematic effects of the characteristics of the plots; it merely ensures that a particular fertilizer is no more likely to be added to a "good" plot than to a "poor" plot.

This raises the issue of the relevance of estimating the "parameter" $(A_2 - A_1)$. It is, in fact, somewhat misleading to call this quantity

a parameter, since it depends on the specific choice of plots. The difference $(A_2 - A_1)$ represents the average effect of adding F_2^* where the average refers to a large number of plantings, with the mix of plots held constant, in the sense that plots with characteristics exactly like those in these five plots are replicated numerous times.

3.7 The Relevance of Average Treatment Differentials

Is this difference of any interest? If these five plots have been chosen randomly from among all plots of conceivable interest, or possibly using a weighted selection process to reflect the importance of different types of plots, then one estimates the average effect of adding F_2^* to plots in general. But note that this estimate will not predict the average increase in yield for any specific plot.

Let us now review the discussion up to this point, and indicate the general significance of these aspects of the experimental environment. Then we shall be in a position to discuss their relevance to educational experimentation. We assume that plant yield relates to three sets of factors: the amount and type of fertilizer applied to the plot (the "treatment"); the systematic characteristics of different plots, such as mineral content and drainage properties which would influence the yield in the same way if the experiment were replicated on the same plot with the same treatment; and the haphazard factors which vary from experiment to experiment. Let us term these three sets the "treatment" variables, the "covariates", and the "noise" variables.

We assume a very general relationship between yield and these three sets of factors; in general, we write:

$$Y = G(F_2, F_3, F_4, F_5; M, D, \ldots; e^1, e^2, \ldots)$$

where the F's represent the levels of the various treatment, M and D are representative covariates, and the e's are representative noise variables. The discussion above indicates the basic rationale for randomization in this context; if the F's are applied to different plots in some arbitrary manner, then a treatment which is used in a plot which has covariates which are particularly favourable will appear to have a larger effect than if the same treatment had been applied to an "average" plot.

By randomizing, one ensures that one treatment has no higher chance to be associated with a "favourable" set of covariates than another treatment. However, one does not eliminate the effects of the covariates; it merely ensures that the effect of a treatment which one expects to estimate, before performing the experiment, is an average of the treatment effects one would infer by repeating the experiment a large number of times on each plot. That is, the expected treatment effect for the j^{th} treatment is $(A_j - A_1)$, where A_j is the average of $E(2)$ as I varies over the five plots, and A_1 is the average yield with no fertilizer, $E_I(1)$, also as I varies over the five plots.

Then the issue becomes: are the averages $(A_j - A_1)$ of interest? This relates to the implicit purpose of the experiment, and to the overall experimental environment, in the sense that the value of specific inferences must relate to the actual choices one can make based on them.

3.8 Alternative Objectives for an Experiment

To clarify these points, consider the following alternative implicit purposes for the overall experiment:

I. On the basis of the experiment, one treatment (fertilizer) is to be chosen, which will be applied on all plots in the future.

II. On the basis of the experiment, treatments will be chosen for all plots, but the choice may be different for different plots.

As we argue below, these may not be the only relevant alternatives, depending on the decision environment. In any case, the relevance and the optimality of different procedures depends on the followiing aspects of the environment:

a. Can the covariates be observed?

b. Can the experiment be replicated, either simultaneously on each plot with different treatments, or sequentially?

c. The functional structure of the response function G.

The relationships among these aspects of the environment, the optimality of different procedures, and the best design of an experiment (or of a series of experiments) is extremely complex. In order to try to delineate different issues, let us survey some of the different cases, and then discuss their relevance to educational experiments.

3.9 Experimental Objectives with Unobservable Covariates and No Replication

Consider first the case where the covariates are not observable. That is, there are systematic factors which relate to the plots but cannot be measured, or at least, such measurement is clearly impractical or uneconomic. That is, for instance, it may not make any difference that the mineral content of a plot could be calibrated exactly, at a very high cost, if the value of having such a measurement is clearly a lower order of magnitude. Thus the assumption is really that the covariate variables cannot, for practical purposes, be measured.

Suppose, further, that the experiment cannot be replicated, either simultaneously or sequentially. To say the experiment cannot be replicated simultaneously, we mean that it is impossible to apply two treatments to different "sections" of the same plot; plots are too "small" to allow us to simultaneously measure:

$$G(F_2^*,0,0,0;M_3,D_3,\ldots;e_3^1,e_3^2,\ldots)$$

and $$G(0,0,F_4^*,0;M_3,D_3,0,0;e_3^1,e_3^2,\ldots)$$

for instance. Actually, as we shall indicate, this assumption is not particularly crucial.

More importantly, we also assume that the experiment cannot be replicated sequentially. That is, if one first uses treatment two on the third plot, to obtain:

$$Y = G(F_2^*,0,0,0;M_3,D_3,0,0,\ldots;e_3^1,e_3^2,\ldots)$$

and then, with a new crop, applies treatment four to the same plot to obtain:

$$Y^* = G^*(0,0,F_4^*,0;M_3,D_3,0,0,\ldots;{}_*e_3^1,{}_*e_3^2,\ldots)$$

then we assume that Y^* is definitely <u>not</u> what one would have obtained had F_4^* been applied at the first planting, even if the haphazard factors had been duplicated (that is, even if ${}_*e_3^j = e_3^j$ for all j). That is, we assume that G^* is a different function than G.

Thus, we implicitly assume that applying a treatment (other than the "control" treatment when no fertilizer is applied) causes a permanent change in the nature of the plot. In fact, for our purposes, let us make the extreme assumption that once one fertilizer is applied, it remains permanently at the same level of concentration, and that furthermore two fertilizers can never be mixed. That is, we assume that once one fertilizer has been applied to a particular plot, it becomes impossible to then change the nature of the plot and apply a different type of fertilizer; furthermore, that procedures which involve "mixing" two types of fertilizer are not relevant.

Although this may seem unrealistic in the agricultural context, note that it does reflect one aspect of the educational environment. Once one has had a specific course - and assuming that reasonable success was achieved - there is a permanent change in the student. Now it is clearly not the case that this permanence is perfect and that the knowledge or understanding does not decay. Furthermore, it is also hardly the case that either simultaneous or sequential applications of different treatments to students is impossible. But the primary reason that the "no replication" assumption is the most appropriate first approximation to develop an analogous case is that the problem, in the educational experiment, is to find the best treatment to apply to <u>other</u> subjects than those in the experiment. If one can replicate the experiment in the agricultural context, then it is important to note that the best procedure may involve some experimentation with <u>each</u> plot, not just those in a pre-designed experiment.

3.10 Experimental Objectives with Unobservable Covariants and Replication Possible

Even in the case where covariates cannot be observed, if replication were possible on each plot, it could be that the best overall procedure

would include some experimentation on every plot used. Instead of using the initial experiment to choose a single fertilizer which would then be applied to all plots, one could choose the treatment to be applied "permanently" to each plot after doing some experimentation on each. This possibility does not necessarily render an initial experiment irrelevant. It could be, for instance, that on the basis of an initial experiment, it is found that two of the fertilizers dominate the other alternatives; regardless of the nature of the plot, for instance, it may be that the best choice is always either treatment two or treatment four. An initial experiment could then imply that the best strategy is to test these two on each plot. Furthermore, if replication is possible sequentially, then for each plot one can estimate the actual treatment effect for that plot, rather than simply the average treatment differential for the plots in the experiment. By replicating the experiment with treatments 3 and 1 on plot 4, for instance, one can obtain an estimate for:

$$E_4(3) - E_4(1)$$

rather than an estimate for $(A_3 - A_1)$ which contains, *ex post*, a plot bias.

In other words, if the treatments can be replicated for each plot, then one can obtain estimates for the differential treatment effects on each plot, and not just estimates of the averages differential, as averaged over the plots in the experiment.

Why is this relevant to the present discussion? As we note above, generally one assumes that a treatment (course) cannot be replicated on a plot (student) and treat this as if it is a "fresh" esperiment. But suppose that one draws the analogy <u>not</u> between experimental plots and <u>students</u>, but between experimental plots and <u>courses</u>. Then one can give the same course over again at a particular university; in this case, the "noise" variables include the characteristics of the students, while the covariates refer to systematic influences of the particular university in question. We shall discuss further the complexities of applying the experimental notions to the educational context below; suffice it to say that there is not just one analogy one can draw between the "classic" agricultural experiment and an educational experiment, and that in fact, an educational experiment, properly viewed, has elements of several different types of agricultural situations. Thus it is useful to consider these different types of experimental environments and then to discuss their relevance to the educational experiment context and their implications for experimental design.

Let us therefore consider some different cases which could arise in the situation where covariates are not observable, but where the experiment can be replicated either simultaneously (a "plot" is "large enough" to accommodate more than one treatment without each sub-plot interfering with the others) or sequentially (where it is assumed that there are no permanent treatment effects, and that if two treatments are used sequentially on the same plot, the order of usage would not affect their expected yield).

In this case, one could obtain estimates of the yield with each combination of treatment and plot, which could be made as reliable as desired by repetition of each combination. In the present context, there are thus 25 numbers of interest. Suppose that one obtains the following pattern after a large number of replications with each treatment on each plot:

Treatment	Plot A	B	C	D	E
1	12	13	11	10	14
2	22	24	23	25	21
3	31	33	32	34	30
4	41	44	42	43	41
5	21	23	22	24	22

Then if the variance of these estimates is small, it is clear that Treatment 4 dominates all the others. There is no need to experiment with each plot in the future, if one is confident that these five plots are representative. Since plots have intrinsic differences, the reliability of such "policy" conclusions depends on whether one has drawn a sample which can validly support such a generalization; clearly validity depends on both the method of selecting the sample and on its size. At this point, one faces a circularity: the necessary sample size for plots depends on their variability in terms of the values of the relevant covariates and on their influence on yield. But the covariates are unobservable and measuring their influence is done within the experiment; clearly the best that can be done is to estimate the variability of yield across different plots after doing the experiment, and then to evaluate the expected improvement in reliability by adding a further sample.

To give one other illustration, suppose one obtains the following pattern:

Treatment	Plot A	B	C	D	E
1	42	21	33	44	21
2	23	13	24	21	12
3	32	4	46	13	13
4	22	13	21	12	44
5	23	12	20	15	22

Here the conclusions to be drawn from the initial experiment are not as transparent. Assuming as before that each of these entries represents a large number of replications with each combination of plot and treatment, so that we can assume that ignoring the possible errors in each estimate is valid, then a careful inspection of this table indicates that treatments 1, 3 and 4 dominate the other two treatments. That is,

if we assume that all plots are represented by one of these five types, then for each type of plot, the best treatment is one of those three (specifically treatment 1 for plots of type A and D, treatment 3 for plots of type B and C, and treatment 4 for a plot of type E). This example indicates the obvious difficulty which arises when the effects of different treatments do not follow a reasonably consistent pattern, as in the first example; the assumption that the sample of plots is representative becomes much more crucial.

3.11 Optimal Experimental Strategies with Unobservable Covariates

Clearly the nature of an optimal combination of an initial experiment, possible follow-on experiments, and plot-specific experimentation is an extremely complex problem in the case where the plot covariates are unobservable yet still have systematic and significant effects on yields. The optimal strategy will depend on variability among plots, the significance of the covariate effects, and the value of identifying the best treatment for individual plots. But it is very important to note several lessons from this case. Even though plots are not distinguishable in the sense that covariates are assumed to be unobservable, it does not follow that the best strategy is necessarily to choose a single treatment which will be applied to all plots. As long as one can apply more than one treatment to a plot, then where the problem is to find the treatment for each plot which should best be applied a large number of times, it may be that some individual experimentation with each plot is part of the optimal overall strategy.

But it does not follow that one should therefore take each potential plot and try each treatment. Initial experimentation may indicate that some treatments dominate. Furthermore, it can reveal the variability of yields and the composition of yield variance in terms of differences in plots and differences in treatments. This information can be used to design the best strategy to be used with each plot. For instance, with one set of estimated parameters, it could be that the best strategy for each plot is described as follows: try Treatment 2; if the yield exceeds a specified level, accept it; otherwise try Treatment 4 and then choose the better of the two. For another set of estimated parameters, the best strategy might be: try Treatments 2, 3 and 5 twice each, then pick that one which has the highest mean yield.

Employing the analogy with the educational context in which "plots" are universities and "treatments" are different curricula for a course, these notions clearly apply, where covariates refer to attributes of the university which are not directly observable. But note that another interpretation exists, in which "plots" are teachers, and "treatments" are course curricula. In this case, the unobservable covariates are now attributes of individual teachers.

When the notion of "plot" is applied to the university or to the teacher, then one is in a situation where different treatments can be replicated, although in the second case it becomes dubious to assume that the "plot" is left unaffected by the "treatment" (that is, a

teacher who uses a particular curriculum is not equivalent to a teacher who has not used that curriculum). But if we consider these notions in a context where the "plot" is the individual student, then, as noted above, the better initial assumption is that it is impossible to replicate treatments. Clearly once a student has had a particular course, he is not equivalent to a student who has not had it. Let us then briefly consider the case where a treatment cannot be replicated on a specific plot.

If treatment replication is not possible, then individual experimentation with each plot becomes impossible. Thus if all covariates are unobservable, the only relevant strategy, after the experimentation phase is complete, is to choose a single treatment which will be applied to all plots. In this case, the problem becomes that of choosing a representative sample of plots on which to experiment and then to choose a criterion by which to select the recommended treatment. Without digressing too far into statistical decision theory, note that the obvious candidate for a selection criterion - to choose the treatment with the highest mean yield - implicitly presupposes a particular loss function and attitude towards risk and uncertainty. It could be, for instance, that a treatment with a lower mean but also a smaller variance might be preferred to the treatment with the highest estimated mean.

This consideration certainly applies to the educational context. Suppose Treatment 2 produces an average "yield" among students randomly chosen equal to an average score of 80, but 10% of students fall below a score of 40, while Treatment 3 produces an average score of 85 but 20% of the students fall below a score of 40. It is not at all clear, if there is no way to discriminate among students *a priori*, which should be the recommended "treatment". Of course, there may be asymmetries at the "high end" as well; treatments may differ in the proportion of students who appear to have outstanding success. Deciding on the proper tradeoff becomes an issue of educational philosophy, perhaps ideology. If more than one measure of treatment yield are allowed, these problems become still greater.

3.12 Experimental Objectives with Separable Treatment and Covariate Effects

The assumed context of the previous discussion is one in which covariates are not observable and in which there are no restrictions placed on the structural form of the function which determines yield. That is, we assume that the relationship between yield and the treatment variables, covariates, and noise variables is given by:

$$Y = G(F_2, F_3, F_4, F_5; M, D, \ldots; e^1, e^2, \ldots)$$

and we have made no assumptions about the complexity of the function G. As the second example above illustrates, we have allowed the impact of each treatment on each plot to be independent, in the sense that knowing some of the plot-treatment yields provides no means to infer the yield from other plot-treatment combinations.

If some structure is assumed, of course, the experimental environment is changed; if severe restrictions on the functional structure of G are assumed, then the context may be changed drastically. Let us begin with the most extreme case, which is to assume that the effects of treatment are additive. That is, suppose that we can write the function G as follows:

$$G(F_2,F_3,F_4,F_5;M,D,\ldots;e^1,e^2,\ldots) =$$
$$H(F_2,F_3,F_4,F_5) + h(M,D,\ldots;e^1,e^2,\ldots)$$

In other words, the effect of the treatments is a function of the fertilizer which can be added to the effect of the covariates and the noise variables. If we define:

$$\beta_1 = H(0,0,0,0)$$

$$\beta_2 = H(F_2^*,0,0,0)$$

and so forth, and:

$$\nu_i = h(M_i,D_i,\ldots;e_i^1,e_i^2,\ldots)$$

then we can write the yield for the i^{th} plot when the j^{th} treatment is applied as:

$$Y_{ij} = \beta_j + \nu_i$$

If we then let decompose ν_i into its mean and the deviation from its mean as the noise variables vary:

$$\nu_i = E(\nu_i) + \varepsilon_i = \eta_i + \varepsilon_i$$

we can write:

$$Y_{ij} = \beta_j + \eta_i + \varepsilon_{ij}$$

where we append both indices to the random component to indicate that it relates to the random component of Y_{ij}. The equation relating Y_{ij} to plot and treatment characteristics now contains two parameters and a random component.

This restriction has implications at two levels. The first is the question of estimating the effects of different treatments. Rather than allowing the 25 combinations of plot and treatment to take on 25 arbitrary values, the expected yield on these 25 combinations can now be characterized with 9 parameters (one of the 10 parameters defined above must be arbitrarily set in order to normalize the two sets of measures; the usual normalization would be $\beta_1=0$). Thus it is not necessary to estimate every plot-treatment combination.

More fundamentally, this specification implies that <u>the optimal procedure with regard to the choice of treatment is necessarily that one treatment should be applied to all plots</u>. That is, the maximization of yield, for <u>every</u> plot, is obtained by choosing the treatment

which maximizes

$$H(F_2,F_3,F_4,F_5)$$

over all feasible treatments. In fact, the previous statement is true regardless of whether treatment effects are linear, as long as treatment effects can be represented as a function which does not depend on the other relevant factors. That is, as long as the function G is separable, in the sense that one can write:

$$G(F_2,F_3,F_4,F_5;M,D,\ldots;e^1,e^2,\ldots) =$$

$$g(H(F_2,F_3,F_4,F_5),h(M,D,\ldots;e^1,e^2,\ldots))$$

then it will follow, as long as the functions H and h can be defined so that an increase in either increases g, that a single treatment is necessarily the best choice for all plots. For instance, one example would be where the effect of applying the i^{th} treatment is to multiply the yield by a constant.

Where the treatment effects are separable from the effects of the other factors, in the sense defined above, then the purpose of the initial experiment is simply to identify the best treatment. In this case, the estimation of plot effects is of interest only to the extent that their estimation enhances the estimation of the treatment effects. Thus in the case in which plot and treatment effects are assumed to be additive:

$$Y_{ij} = \beta_j + \eta_i + \epsilon_{ij}$$

the η_i are sometimes called "nuisance parameters" since their estimation is merely a means to an end.

3.13 Experimental Objectives with Observable Covariates

We now consider the case in which covariates are observable. In this case, it is easier to articulate the experimental design issues by beginning with the most restrictive assumptions about the nature of the covariate effects, in terms of restrictions on the functional form of the function G, and then analyzing the effect of gradually relaxing these restrictions.

We thus assume that for each plot, we have measurements for characteristics such as mineral content and drainage properties which affect yield. These specific examples are convenient, since they also can plausibly represent another important case: that in which some covariates are observable (e.g. mineral content) and some are unobservable (e.g. drainage properties). While we shall note the implications of unobserved covariates at certain points, we assume here that all relevant covariates are observable. Thus for the i^{th} plot, we assume that we have measurements M_i, D_i, and so forth, for a finite number of covariates.

Consider first the case where one accepts the restriction that the treatment effects and the covariate effects are additive; that is,

the yield Y_{ij} for the j^{th} treatment on the i^{th} plot can be written:

$$Y_{ij} = \beta_j + \eta_i + \epsilon_{ij}$$

where:

$$\eta_i = E(h(M_i, D_i, \ldots; e_i^1, e_i^2, \ldots))$$

and where we now assume that M and D are observable.

Now if the experiment can be replicated on each plot, in the sense defined above, then the β_j can be estimated by applying each treatment to each plot; accuracy can be attained by repeating each treatment a large number of times on each plot, if necessary. Note once again that given the assumption that the treatment effect is additive, then the purpose of the experiment is necessarily to find the best treatment, which is necessarily the best treatment irrespective of plot characteristics.

Since each treatment can be replicated as often as necessary on each plot, randomization across plots is not at issue; however, randomization of the order in which different treatments are applied to each plot would still be in order, to protect against the distinct possibility that there are some lingering effects of applying a treatment to a specific plot.

Now consider the case where replication on a plot is impossible; as noted, this case relates to the interpretation of "plot" as "student" and "treatment" as "course". With replication impossible, the assumption that all relevant covariates are observable becomes crucial.

Thus assume first that all relevant covariates are observable, and thus that the only unobservable factors are the haphazard "noise" effects. In this case, if one is able to find an arbitrarily large number of plots with any specific combination of covariates, then the previous case essentially applies. If all relevant covariates are observed and one can find as many plots as needed with any specific combination of covariates, then in essence the experiment can be replicated by applying the various treatments to plots with the same combinations of covariates.

3.14 Restrictions on the Assumed Response Surface

If it is not costless or feasible to obtain arbitrarily numerous duplications of plots with identical covariate characteristics, the situation becomes far more complex. If absolutely no restriction is placed on the character of the function h, which defines the effects of the covariates, then the situation is essentially equivalent to the case in which covariates are not observable. It is important to stress this conclusion; the price of maintaining a totally agnostic approach with regard to the "response function" $h(M,D,\ldots;e^1,e^2,\ldots)$ is that the observation of the covariates becomes irrelevant. By refusing to allow inferences about the effects of the covariates for one (M,D,...) combination about other covariate combinations, the

observation of the covariates is then irrelevant, except in the limited case in which plots with precisely the same covariate combinations can be obtained.

In virtually all situations, the analyst is at least willing to assume some form of "smoothness" for the response function; that is, it is accepted that for covariate combinations which are sufficiently "near" each other, the covariate effects are approximately equal. Assuming that covariate "neighborhoods" produce roughly similar effects, then a natural procedure is to distinguish intervals for each covariate, and then to group plots in which all covariates fall within the same interval. That is, division points are arbitrarily selected:

$$M_1^*, M_2^*, M_3^*, \ldots\ldots M_P^*$$

$$C_1^*, C_2^*, C_3^*, \ldots\ldots C_Q^*$$

and then the same expected yield is assumed to apply to all plots for which:

$$M_i^* \leq M < M_{i+1}^*$$

$$C_k^* \leq C < C_{k+1}^*$$

We shall refer to each combination of intervals, in which one interval is specified for each covariate, as a "cell".

Note that this procedure has three effects. First, it assumes that covariate effects are discontinuous and are constant within each "cell". Thus consider two plots which share precisely the same value for C, and which have values for M which are very "close" but which fall into different intervals. This procedure assumes that the expected yield for these two is equal to the cell means in their respective cells, whereas common sense suggests that their expected yield should be approximately equal, based on the fundamental "smoothness" assumption.

Second, this procedure implies that there must be at least two plots in every cell for which observations are made, since a single observation on a cell provides information only as to the "plot effect". Thus if it is difficult to find plots with "similar" values of the covariates, there will be a strong tendency to impose wide intervals, which exacerbates the first problem mentioned.

Third, this procedure implicitly allows the treatment effects of different cells to be completely unrelated. No structure is imposed, and thus it is not possible to devise a test of whether this general approach is valid. Of course, in a sense this procedure is always valid since the intervals can theoretically be made small enough that the range of covariate effects within a cell is within an arbitrarily small band, given the "smoothness" assumption. But it is possible neither to test whether the intervals are too broad nor to test whether there are simple systematic relationships in the covariate effects among different cells without imposing more structure on the function h.

In general, if the function h does have a reasonably simple structure and the analyst can guess the general form of this structure (that is, the analyst can devise a "functional form" which can be parameterized to approximate the actual structure), then more efficient estimates can be obtained by imposing the relevant restrictions.

3.15 Imposing Restrictions on the Response Surface: The Linear Case

In the simplest case, suppose that covariate effects are linear. That is, suppose that one can write the function h (·) as follows:

$$h(M,D,\ldots;e^i,e^2,\ldots) = \alpha + \lambda M + \nu C + \ldots + g(e^1,e^2,\ldots)$$

$$= \alpha + \lambda M + \nu C + \ldots + \varepsilon$$

so that the equation defining yield on the i^{th} plot can be written:

$$Y_{ij} = \beta_j + \alpha + \lambda M_i + \nu C_i + \ldots + \varepsilon_{ij}$$

The yield equation is now in the form generally associated with an "analysis of covariance", except that the normalization required to make all parameters estimable has not been imposed (that is, clearly one cannot estimate both the parameter α and a treatment effect β_j for all treatments). It is useful to note that this equation can be immediately translated into a linear regression by the device of defining indicator variables ("dummy variables", "binary variables") which show the occurrence of the j^{th} treatment. That is, let us define the variable D_j as follows:

$$D_j = \begin{cases} 1 & \text{if the } j^{th} \text{ treatment has been applied to the plot in question} \\ 0 & \text{otherwise} \end{cases}$$

Then we can rewrite the analysis of covariance equation in terms of variables which all apply to the i^{th} plot (the i^{th} "observation" in linear regression terms) as follows:

$$Y_i = \Sigma_{j=1}^{J} \beta_j D_j + \alpha + \lambda M + \nu C + \ldots + \varepsilon$$

3.16 Nonlinear Response Surfaces

The important aspect of the equation above is that the treatment effects and the covariate effects are independent. Within this context, we can generalize the form of the function h (·) without changing the qualitative nature of the specification. For instance, the effect of covariates could be assumed to be nonlinear in various ways. The introduction of quadratic terms:

$$Y_i = \Sigma_{i=1}^{J} \beta_j D_j + \alpha + \lambda M + \theta M^2 + \nu C + \mu C^2 + \ldots + \varepsilon$$

or "interactive" terms:

$$Y_i = \Sigma_{i=1}^{J} \beta_j D_j + \alpha + \lambda M + \delta M \cdot C + \nu C + \ldots + \varepsilon$$

or both, is the most natural generalization of the functional form. However, insight into the nature of the process may imply still other function forms which are more appropriate such as:

$$Y_i = \sum_{i=1}^{J} \beta_j D_j + \alpha + \lambda M + \tau(1/M) + \omega(\log C) + \ldots + \varepsilon$$

Obviously there are an infinity of ways in which a functional form can be chosen. In theory, the choice of a functional form should represent the analyst's insights about the "true" nature of the relationships while providing enough flexibility to encompass all plausible shapes for the "response surface".

Of course, since the analyst rarely can honestly impose significant restrictions on the basis of *a priori* knowledge or theory, the implied precept is that the functional form should retain a high degree of flexibility. Whether retaining flexibility is "costly" depends primarily on the number of observations which will be available. This presents another circularity: the value of more or fewer observations also depends on the complexity of the functional form. As in other aspects of the optimal experimental design problem, once-and-for-all choices appear to be clearly dominated by a sequential design, in which information obtained in the early phases is applied to the design in later phases.

3.17 Inevitable Approximation in Specifying the Response Surface

We have been discussing possible specifications for the covariate effects, and have noted that the functional form may have a variety of specifications. In fact, it is clear that the functional form will necessarily be an approximation. Assuming that, generally, the "true" functional specification will be an arbitrary function - possibly very smooth - which cannot be exactly described either as a polynomial or in terms of a simply defined rational function, the question becomes: what is the relevance of the inevitable error in functional specification? That is, except for a few physical processes, for which theory may imply a specific additive or multiplicative effect (even here, these specifications are almost always approximations), the true specification will not conform to any simple algebraic function. Thus the estimation of the relationship with a simple algebraic function necessarily imposes an error from the approximation. The issue becomes what the relevance of this error is.

Suppose in general that the true relationship is:

$$Y_{ij} = \Sigma_{j=1}^{J} \beta_j D_j + h(M_i, C_i, \ldots) + \varepsilon_{ij}$$

while the analyst assumes the relationship:

$$Y_{ij} = \Sigma_{j=1}^{J} \beta_j D_j + H(M_i, C_i, \ldots) + \varepsilon_{ij}$$

Let us define the variable τ_i as the value of the discrepancy for the i^{th} plot:

$$\tau_i = h(M_i, C_i, \ldots) - H(M_i, C_i, \ldots)$$

Then, in a sense, the τ_i operate like plot effects where the covariates are not observed. They impart systematic biases, although the nature of the bias for each plot actually depends on the entire sample (that is, the discrepancies between the estimated function h and the estimated function H depends on the nature of the sample with regard to the values of the covariates). The only way these systematic biases can be definitely overcome is by imposing balance on the experimental design, in the sense that, for each treatment, the same distribution of the covariates exists across observations for which the treatment in question is applied.

That is, the point is that the function which defines the covariate effects cannot be specified exactly; an approximation in functional structure is inevitable. This approximation will produce an error - the nature of which depends on the complete distribution of covariate combinations across the observations - which will then affect the treatment effects. However, if for each treatment, the same set of covariate combinations is present, then there is no bias in the estimates of the treatment effects. Thus to be perfectly certain that there is no bias due to the approximate specification of the functional form for the covariate effects, one must impose a "perfectly balanced" design in terms of the covariates.

3.18 Interaction between Treatments and Covariate Effects

Now let us turn to the related questions of the specification of the treatment effects, and particularly of the overall effect of treatments and covariates. We have implicitly assumed away problems of specifying treatment effects, by assuming that just one "level" of treatment is relevant, for each "type" of treatment. Formally, different treatments, as we have defined them, could refer to different "levels" of the same "actual treatment". Thus, treatments 2 and 3 could refer to the applications of one and two pounds of Fertilizer Type B, for example. Where one wishes to treat the possibilities of continuous applications, the situation becomes far more complex.

Fortunately, in the educational context, each curriculum represents a distinct treatment, and continuous variation is not meaningful. Thus if "essentially" the same course is taught in a ten week quarter and in a fifteen week semester, the only reasonable approach is to treat these as distinct treatments, although some analysis of the outcome of teaching the "same material" in ten or fifteen weeks is still possible.

We thus continue to assume that a set of distinct treatments are considered, and do not worry about the problems arising if continuous variation in treatment level is possible. Thus it follows that we can continue to specify treatment variables as binary variables which indicate the presence or absence of a particular treatment. As explained above, we also continue to assume that at most one treatment can be applied to a particular subject.

Even though the specification of treatment effects can therefore be

handled as binary variables, and even if we assume that the proper specification for the covariates is used, a crucial functional specification issue remains. This is the question of determining the interactions between treatment effects and covariate effects.

Our discussion to this point has considered the case where the set of treatment effects and the set of covariate effects are separable, in the sense that:

$$Y_{ij} = \Sigma_{j=1}^{J} \beta_j D_j + h(M_i, C_i, \ldots) + \varepsilon_{ij}$$

In other words, the effects of covariates separate from the effects of treatments: the effect of a change in the covariates is the same, regardless of which treatment is considered, and the differential effect of different treatments is the same, regardless of which combination of covariates is considered.

Clearly this separability property may not hold. The effect of mineral content may differ, depending on which fertilizer is added. Furthermore, adding a particular fertilizer may have a different effect, depending on the drainage properties of the plot in question.

In the case of educational experimentation, the possibility that covariate and treatment effects may not be separable is clear, and its implications are obviously significant. In that context, taking students and faculty characteristics as the observed covariates and the course curricula as the treatments, the analogous point is that the effect of different curricula may well depend on the characteristics of the students and the faculty. It follows that for different students, and for different faculty, different curricula may be optimal.

The typical method by which the separability assumption is relaxed is to introduce product terms into a linear specification. Thus instead of the specification:

$$Y_{ij} = \Sigma_{j=1}^{J} \beta_j D_j + \lambda M_i + \nu C_i + \ldots + \varepsilon_{ij}$$

one introduces the terms D_jM_i, D_jC_i, and so forth:

$$Y_{ij} = \Sigma_{j=1}^{J} \beta_j D_j + \lambda M_i + \nu C_i + \ldots + \Sigma_{j=1}^{J} \delta_j D_j M_i + \Sigma_{j=1}^{J} \theta_j D_j C_i + \ldots + \varepsilon_{ij}$$

However, it should be noted that introducing cross-product terms is not the only method for allowing "interaction". In general, this is a natural method, since it allows the same general functional specification with regard to covariate effects for each treatment.

Thus if the initial separable specification is:

$$Y_{ij} = \Sigma_{j=1}^{J} \beta_j D_j + \lambda(\ln M_i) + \nu(1/C_i) + \varepsilon_{ij}$$

then given treatment K, for instance, the specification implies that:

$$Y_{iK} = \beta_K + \lambda(\ln M_i) + \nu(1/C_i) + \varepsilon_{ij}$$

If we introduce all cross-product terms, to give the specification:

$$Y_{ij} = \Sigma_{j=1}^{J}\beta_j D_j + \lambda(\ln M_i) + \nu(1/C_i) + \Sigma_{j=1}^{J}\delta_j D_j(\ln M_i)$$

$$+ \Sigma_{j=1}^{J}\theta_j D_j\ (1/C_i) + \varepsilon_{ij}$$

then the specification, <u>given</u> treatment K, is:

$$Y_{iK} = \beta_K + (\lambda+\delta_K)(\ln M_i) + (\nu+\theta_K)(1/C_i) + \varepsilon_{iK}$$

That is, by allowing all cross-product terms, the same functional specification is made for each treatment level, but all of the parameters are allowed to be different for each treatment.

Clearly one can relax separability in other ways: by introducing terms which are not cross-products, and also by introducing some cross-product terms, but not all.

3.19 Treatment-Covariate Interaction and Experimental Design: A Neglected Issue

By introducing a partial set of cross-product terms, the possibilities for allowing treatment effects to depend on covariates, and *vice versa*, are introduced without dramatically increasing the difficulty of the estimation problem. If a complete set of cross-product terms is introduced, then one has transformed the problem into one in which a separate equation is estimated for each treatment level. In other words, a complete set of cross-product terms transforms the specification into one in which information obtained for one treatment has no relevance for inferences about other treatments. Thus the general precept should be to introduce those cross-product terms - and possibly other terms which involve both treatment variables and covariates - which seem necessary to reflect significant interactions.

It is recognized that interaction terms should be introduced where it is plausible that treatment effects and covariate effects interact, so that the estimating equation can be properly specified. Only if the estimating equation is properly specified can one be assured that estimates of the β_j's, the "main treatment effects", are unbiased. While true, <u>this view of the problem is fundamentally in error</u>. That is, many analysts continue to assume that estimation of the β_j is the objective of the experiment, and that the other parameters are simply "nuisance" parameters, whose only role is to improve the estimation of the β_j.

But as we have noted, only in the case in which separability is assumed does it follow that the object of the experiment is necessarily to find the <u>one</u> treatment which should be applied to all "plots". When the specification is not separable, then the best treatment to be applied to a particular plot depends on the covariates of the plot in question. If the covariates are observable, as we assume here, then one can obviously tailor the treatment to the subject. As we note above, it

may be possible to tailor treatments to subjects even where covariates are not observable in the case where different treatments can be applied to the same plot in sequence.

With interaction, the problem is to find the treatment which is best as a function of the covariates. In other words, in contrast to the relatively simple problem of finding the "best" treatment, in the case in which treatment effects are separable, the problem is how to define those "boundaries" in "covariate space" which separate regions in which different treatments are optimal.

This is an exceedingly complex problem. The standard experimental design literature has not addressed this problem, and we are searching the literature for useful analyses. Once the assumption is relaxed that there is a single treatment which is best for all subjects, and the object of the experiment is simply to determine which treatment is the best one, the possibilities become very great, and depend on the functional form which is chosen to define the general structure of treatment and covariate effects. As the discussion above, concerning possible optimal strategies where unobserved covariates, indicates, there are a variety of different approaches.

In the context of educational experimentation, it is clear that the presumption of a single best curriculum for all students and all teachers is extremely dubious. The problems of designing an experiment in which the best treatment is identified, as a function of covariates, have not been thoroughly investigated. We are proceeding with an analysis of these issues, and seeking ways to adapt classical experimental procedures to this problem.

4 PARTICIPATING INSTITUTIONS

COLLEGE OR UNIVERSITY: Allegany Community College

FACULTY LIAISON - PROFESSOR: F.E. Weldon

ADDRESS: Willow Brook Road
P.O. Box 870
Cumberland, Md 21502

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course: 50

TEXTS: McConnell
Heilbroner

COURSE DESCRIPTION: Two semester micro-macro sequence.
Use programmed learning, study guides, texts, film and filmstrips.
A section of 10 used in TV section in connection with Maryland Center for Public Broadcasting. Study is done in the home except tests taken at the college. University of Maryland instructor is on camera.

COLLEGE OR UNIVERSITY: Anchorage Community College--University of Alaska

FACULTY LIAISON - PROFESSOR: Larry L. Ross

ADDRESS:

Department of Economics
Anchorage Community College
University of Alaska
2533 Providence Avenue
Anchorage, Alaska 99504

NUMBER OF STUDENTS:

Micro Course: 80-100 per semester
Macro Course: 150-180 per semester
Combined Course:

TEXTS: McConnell
Supplemental paperbacks.

COURSE DESCRIPTION: Class meets twice a week for 1½ hour sessions.
Average class size is 25-30.
Two semester course.
No teaching assistants used.

COLLEGE OR UNIVERSITY: Bakersfield College

FACULTY LIAISON - PROFESSOR: Richard Grass and Clyde Verhine

ADDRESS: Bakersfield College
1801 Panorama Drive
Bakersfield, CA 93305

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course:

TEXTS:

COURSE DESCRIPTION:

COLLEGE OR UNIVERSITY: Ball State University

FACULTY LIAISON - PROFESSOR: David T. Nelson

ADDRESS: Department of Finance and Management
College of Business
Ball State University
Muncie, Indiana 47306

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course:

TEXTS:

COURSE DESCRIPTION:

OTHER INFORMATION: Interested in working on the instruments to be used for collection of data and the research design used to evaluate the data to measure the effectiveness of the different techniques.

COLLEGE OR UNIVERSITY: Boise State University

FACULTY LIAISON - PROFESSOR: Lamborn, Asmus, Billings, Payne Mitchell, Holley, Eastlake

ADDRESS: Boise State University
1910 College Boulevard
Boise, Idaho 83725

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course: 400-500/semester

TEXTS: Spencer

COURSE DESCRIPTION:

Two semester sequence (macro then micro).
Classes meet MWF for 1 hour or T,Th for 1 hr. 15 min.; some sections meet for 3 hours one evening per week.
Class size is 20-40 students with 2-3 large sections each semester of 100-200.
Classes taught individually by the faculty with no assistants.
Standard lecture with some experimentation with the personalized system of instruction and the use of behavioral objectives.
Some use of simulation models.

COLLEGE OR UNIVERSITY: Boston University

FACULTY LIAISON - PROFESSOR: Blanche Fitzpatrick

ADDRESS: Economics Department
270 Bay State Road
Boston, Massachusetts 02215

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course: 1800

TEXTS: Samuelson

COURSE DESCRIPTION: Regular faculty have 1 section; teaching fellows have 2;
Meet 3 hours a week
Class size 30-40- or 50.
Faculty and teaching fellows responsible for their own sections.
Two semester course.

OTHER INFORMATION: Proposed for 1975-76:
Regular faculty to have 1 100-student section.
Teaching fellows to have 2 25-30-students sections.
Faculty will teach twice a week--class will meet in sections with junior teaching fellows once a week.
Senior teaching fellows will have smaller classes, no sections.
Possible text change.

COLLEGE OR UNIVERSITY: California Polytechnic State University

FACULTY LIAISON-PROFESSOR: Dominic B. Perello

ADDRESS: Department of Economics
California Polytechnic State University
San Luis Obispo, CA 93407

NUMBER OF STUDENTS:

Micro Course:	400
Macro Course:	400
Combined Course:	625

TEXTS: Spencer, Contemporary Economics

COURSE DESCRIPTION: Class size 40-45; meet for 1½ hour sessions MWF or T, Th.
3 quarter units
Standard lecture format by faculty.
No teaching assistants, but reader is used for homework.

OTHER INFORMATION: Have used modified Attiyeh macro problem in the past, but discontinued because of crowded computer center.

COLLEGE OR UNIVERSITY: California State University, Los Angeles

FACULTY LIAISON - PROFESSOR: George Jensen

ADDRESS: School of Business and Economics
California State University-Los Angeles
5151 State University Drive
Los Angeles, California 90032

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course: 650

TEXTS: Samuelson
Mansfield
Gill
Lynn

COURSE DESCRIPTION: Two quarter micro-macro sequence.
3, 4 and 6 unit courses offered for different majors and levels.
Standard lecture-discussion format.
35-50 students per section.
No teaching assistants used in sections or for grading.

COLLEGE OR UNIVERSITY: Central College

FACULTY LIAISON - PROFESSOR: Donald T. Butler

ADDRESS: Department of Economics
Central College
Pella, Iowa 50219

NUMBER OF STUDENTS:

Micro Course: 180
Macro Course: 180
Combined Course:

TEXTS: Reynolds

COURSE DESCRIPTION: 10-11 week terms; 45 meetings/term.
Use Teaching Assistant in tutorial capacity two days/week
(10% micro, 60% macro students use T.A.)

OTHER INFORMATION: Professors serve in tutorial capacity for disadvantaged and slow students

COLLEGE OR UNIVERSITY: Colgate University

FACULTY LIAISON - PROFESSOR: Frank A. Farnsworth

ADDRESS: Department of Economics
Colgate University
Hamilton, New York 13346

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course: 500

TEXTS: Samuelson, Suits, Spencer, McConnell

COURSE DESCRIPTION:

All Senior faculty teach.
Class size is kept to a maximum of 50.
Two to four sessions/week; one term course.
No teaching assistants are used.
Teaching approach is determined by the individual faculty instructor--varies from very conventional to experimental. Includes Keller approach, Boulding's "Little Green Men" modules, computer model manipulation.
Some require daily use of Wall Street Journal, some use "caselettes" developed for the course, some use tape and video-cassettes developed at Colgate.

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COLLEGE OR UNIVERSITY: College of San Mateo

FACULTY LIAISON - PROFESSOR: Frank Leroi

ADDRESS: College of San Mateo
1700 West Hillsdale Boulevard
San Mateo, CA 94402

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course: 350

TEXTS: McConnell; Levy & Sufrin and student guide, plus Rendig Fels Casebook. Assigned readings in Annual Editions 1974-75.

COURSE DESCRIPTION: Two semester, macro-micro sequence.
3 hours per week.
Lecture plus discussion/quiz section.
No teaching assistants or readers.

OTHER INFORMATION: Each instructor chooses his own text and supplementary materials. Only coordination is that the first semester covers macro, the second, micro.

COLLEGE OR UNIVERSITY: Creighton University

FACULTY LIAISON - PROFESSOR: Thomas O. Nitsch

ADDRESS: College of Business Administration
Creighton University
2500 California Street
Omaha, Nebraska 68178

NUMBER OF STUDENTS:

Micro Course:	188
Macro Course:	64
Combined Course:	

TEXTS: Currently Spencer
Previously, McConnell
Adopted by majority vote of Dept. faculty.

COURSE DESCRIPTION: Two one-semester courses required for Bus. Ad. majors and elective for Arts & Sciences majors.
Three lecture sessions per week;
No teaching assistants or tutorial sessions employed.
Main text adopted by majority vote of faculty; may workbooks, programmed texts, etc. at the option of the individual instructor and/or student.
Micro sections range in size from 44-49.
Macro sections range in size from 11-39.

COLLEGE OR UNIVERSITY: Delgado Junior College

FACULTY LIAISON - PROFESSOR: Harwood I. Brown

ADDRESS: 2909 Esplanade Avenue
New Orlenas, LA 70119

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course: 200 to 250

TEXTS: Spencer, Contemporary Economics

COURSE DESCRIPTION:
Class size maximum of 50.
Two semesters of 15 weeks each; meet three hours each week.
No teaching assistants.
Use T.U.C.E. as a pretest and as a post-test for the course.
Recommend use of Wall Street Journal for current reading.
Some use made of Professor Fels' cases developed at Vanderbilt.
Emphasis on current economy.

COLLEGE OR UNIVERSITY: Denison University

FACULTY LIAISON - PROFESSOR: Larry Ledebur

ADDRESS: Department of Economics
Denison University
Granville, Ohio 43023

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course:

TEXTS: Each professor chooses his own text.
Vast variety; tend to be paperback editions.

COURSE DESCRIPTION: Introductory courses offered on a modular system. Students take an initial seven week module which covers traditional micro and macro concepts with basic applications. Thereafter they take two three-week modules. The modules are issue or problem oriented and cover specific topics of interest.

In addition there is a self-paced program of 100 students per semester. Students complete one module in addition to all of the self-paced work.

COLLEGE OR UNIVERSITY: Everett Community College

FACULTY LIAISON - PROFESSOR: Michael T. Kolosseus

ADDRESS: Everett Community College
801 Wetmore Avenue
Everett, Washington 98201

NUMBER OF STUDENTS:

Micro Course:
Macro Course: 100-140
Combined Course: 175

TEXTS: Samuelson plus workbooks and books of readings.
Some programmed work by Bach/Lumsden/Attiyeh.
For Survey course: Heilbroner
NOTE: policy has been to switch from Samuelson to Bach and back every two years.

COURSE DESCRIPTION: Lecture-discussion with workbook and supplements provided by the instructor. Transparencies.
Three sections of macro each quarter. One section macro in the summer.
Average class size of about 35.
Five sections of survey course for non-majors each year.

COLLEGE OR UNIVERSITY: Gavilan College

FACULTY LIAISON - PROFESSOR: Howard Hetzler

ADDRESS: Gavilan College
5055 Santa Teresa Blvd.
Gilroy, CA 95020

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course: 80 per quarter

TEXTS: Books 1,2,3,4,5, & 6 Economic Analysis Series, Sterling Institute

COURSE DESCRIPTION: A self-study course with each student proceeding according to hiw own capabilities.
Required attendance 2 periods / week.
Deadlines for tests to be met by specified dates during the quarter.
Teaching Assistants used for tutoring.

OTHER INFORMATION: Audio-Visual-Tutorial materials are available for students wishing to review and supplement their studies.

COLLEGE OR UNIVERSITY: Georgia State University

FACULTY LIAISON - PROFESSOR: Theodore C. Boyden

ADDRESS: Georgia State University
33 Gilmer Street, S.E.
Atlanta, Georgia 30303

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course:

TEXTS:

COURSE DESCRIPTIONS:

COLLEGE OR UNIVERSITY: Grand View College

FACULTY LIAISON - PROFESSOR: Matilda W. Wilkinson

ADDRESS: Grand View College
1200 Grandview Avenue
Des Moines, Iowa 50316

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course: 100-120

TEXTS: Heilbroner, The Economic Problem, with student study guide.

COURSE DESCRIPTION: Classes meet for three 50-minute sessions per week. Lecture. No teaching assistants.

COLLEGE OR UNIVERSITY: Haverford College

FACULTY LIAISON - PROFESSOR: Holland Hunter

ADDRESS: Department of Economics
Haverford College
Haverford, PA 19041

COLLEGE OR UNIVERSITY:	Heriot-Watt University
FACULTY LIAISON - PROFESSOR:	Keith G Lumsden Donald I MacKay
ADDRESS:	Department of Economics Heriot-Watt University Edinburgh Scotland
NUMBER OF STUDENTS:	Micro Course: 280 per semester Macro Course: 280 per semester
TEXTS:	Samuelson Lumsden, Attiyeh, Bach
COURSE DESCRIPTION:	Class meets Monday, Tuesday, Wednesday for one hour. Two teaching assistants used for tutorials, course assistant and office hours. Video tapes of lectures available for use by students for two weeks following each lecture.

COLLEGE OR UNIVERSITY: Illinois State University

FACULTY LIAISON - PROFESSOR: Douglas Poe

ADDRESS: Center for Economic Education
Illinois State University
Normal, Illinois 61761

NUMBER OF STUDENTS

Micro Course:
Macro Course:
Combined Course: 2000-2500

TEXTS: Samuelson, Mansfield, Lipsey & Steiner, McConnell, Mimeographed materials by staff

COURSE DESCRIPTION: Two semester course: basics and macro first semester, followed by micro the second semester.
Sections meet 3 times/week for fifty minutes or 2 times/week for seventy-five minute sessions.
Sections vary in size from 30 to 500. Most about 35.
Teaching assistants aid in administrative and grading work.

OTHER INFORMATION: Past Spring offered one computer integrated section and several computer assisted sections.
Next fall will initiate Doctor of Arts program which will include internships for advanced candidates. They will be used as supervised instructors in some sections.

Two courses which depart from traditional approach are offered: "Introduction to Economic Science" and "Economics and the Arts"
Also one section of part-time students which uses video cassettes.

COLLEGE OR UNIVERSITY: Indiana University

FACULTY LIAISON - PROFESSOR: Phillip Saunders

ADDRESS: Department of Economics
Indiana University
Bloomington, Indiana 47401

NUMBER OF STUDENTS

Micro Course:	2200
Macro Course:	2200
Combined Course:	

TEXTS: Each instructor chooses own text

COURSE DESCRIPTION:
Sections meet three times/week for 45 minute sessions or two times/week for 75 minute sessions
Most section 45-75 students; one 360 student section.
Each instructor runs his individual section; common final exam.
Micro and Macro sections offered each semester.

COLLEGE OR UNIVERSITY: Iowa State University of Science and Technology

FACULTY LIAISON - PROFESSOR: R.W. Thomas

ADDRESS: Department of Economics
Iowa State University of Science and Technology
Ames, Iowa 50010

NUMBER OF STUDENTS:

Micro Course: Fall, 1,100; Winter, 1,200; Spring, 800.
Macro Course: Fall, 1,500; Winter, 1,100; Spring, 900.
Combined Course:

TEXTS: Spencer, Contemporary Economics,
Workbook recommended but not required.

COURSE DESCRIPTION: Both macro and micro courses offered each quarter.
Sophomore level courses.
No full year course.

OTHER INFORMATION: Experienced a 15% increase in enrollments in the past year. No one expects any more large increases, figures should remain similar to those given above.

COLLEGE OR UNIVERSITY: The Johns Hopkins University

FACULTY LIAISON - PROFESSOR: Carl F. Christ

ADDRESS: Department of Political Economy
The Johns Hopkins University
Baltimore, Maryland 21218

NUMBER OF STUDENTS

Micro Course: 220 per semester
Macro Course: 220 per semester
Combined Course:

TEXTS: Samuelson, no workbook

COURSE DESCRIPTION: Lectures by faculty; section by graduate students.
No programmed learning materials.
Wrote own homework questions and exams
20-25 students per section

COLLEGE OR UNIVERSITY: Lafayette College

FACULTY LIAISON - PROFESSOR: Alfred E. Pierce

ADDRESS: Economics Department
Easton, Pennsylvania 18042

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course: 400

TEXTS: McConnell
Wall Street Journal

COURSE DESCRIPTION: Lectures on Monday to all students given by departmental specialist.
Two discussion sections with individual instructor.
All economics faculty (10) participate in the lecutures and discussion sessions at some time in the course.
Use common exams (two + final), individual quizes, problems and exercises.

COLLEGE OR UNIVERSITY: Lee College

FACULTY LIAISON - PROFESSOR: Henry M. Austin

ADDRESS: Lee College
Post Office Box 818
Baytown, Texas 77520

NUMBER OF STUDENTS

Micro Course: 120-150 per semester
Macro Course: 120-150 per semester
Combined Course:

TEXTS: Bingham's Programmed Textbook
Will change to new standard text in fall of 1975.

COURSE DESCRIPTION: Class size averages 20-25, 35 maximum.
Meet three times/week for 50 minute sessions or two times per week for 75 minute sessions.
No teaching assistants.
Two semester sequence of micro-macro for business and economics students; one semester course for non-majors.

COLLEGE OR UNIVERSITY: Mansfield State College

FACULTY LIAISON - PROFESSOR: Donald C. Darnton

ADDRESS: Mansfield State College
Mansfield, Pennsylvania 16933

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course:

TEXT: Willis Peterson, PRINCIPLES OF ECONOMICS

COURSE DESCRIPTION: Lecture, class discussion, tests.
Case studies, student position papers, class discussion.
Section size about 20.

COLLEGE OR UNIVERSITY: Marietta College

FACULTY LIAISON - PROFESSOR: Richard K. Smith

ADDRESS: Department of Economics, Management and Accounting
Marietta College
Marietta, Ohio 45750

NUMBER OF STUDENTS

Micro Course:
Macro Course:
Combined Course: 225

TEXT: Samuelson, ECONOMICS
Samuelson, READINGS IN ECONOMICS
Robinson, A STUDY GUIDE TO ACCOMPANY ECONOMICS

COURSE DESCRIPTION: Three semester hours credit; three 50 minute sessions/week.
No teaching assistants, but some undergraduates used as graders.
Course is two semester sequence: macro-micro.
There are 3 three hour exams, a minimum of ten homework assignments, and a final examination.

COLLEGE OR UNIVERSITY: Modesto Junior College

FACULTY LIAISON - PROFESSOR: John E. Byrne

ADDRESS: Department of Economics
Modesto Junior College
College Avenue
Modesto, CA 95350

NUMBER OF STUDENTS

Micro Course:	90
Macro Course:	250
Combined Course:	

TEXTS: Gill

COURSE DESCRIPTION:
Course is given in 2 semesters (3 units each);
Each class meets 3 hours/week and contains both lectures and discussions.
Average class size is 40.
Text is supplemented with assigned readings and videotapes.
Class presentations are supplemented with overhead projections of transparencies and videotapes.
No teaching assistants utilized.

OTHER INFORMATION: Computer installation expected in 1975.

COLLEGE OR UNIVERSITY: New York University

FACULTY LIAISON - PROFESSOR: John D. Guilfoil

ADDRESS: College of Business and Public Administration
800 Tisch Hall
Washington Square
New York, New York 10003

NUMBER OF STUDENTS:

Micro Course:	156
Macro Course:	521
Combined Course:	

TEXTS: Samuelson

COURSE DESCRIPTION:

Macro: Meets twice a week for 1 semester
Teaching assistants not used.
Class size 55 - 95

Micro: Meets twice a week for 1 semester
Teaching assistants not used.
Class size 35- 50

COLLEGE OR UNIVERSITY: Prince George's Community College

FACULTY LIAISON - PROFESSOR: J. A. Billings

ADDRESS:

Economics Department
Prince George's Community College
301 Largo Road
Largo, Maryland 20870

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course: 800/semester plus summer session

TEXTS: Bach, McConnell, Spencer, Gill

COURSE DESCRIPTION: Classes meet for total time of 2½ hours per week.
Class size approximately 40.
Five man economics faculty with 4 part-time instructors.
Two basic principles courses plus one course for non-majors and one interdisciplinary course.

COLLEGE OR UNIVERSITY: Rice University

FACULTY LIAISON - PROFESSOR: Kenneth J. White

ADDRESS: Economics and Business Administration
Rice University
Houston, Texas 77001

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course: 700 per year

TEXTS: Samuelson
Various Paperbacks

COURSE DESCRIPTION: Two courses, one is concentrated theory course; one is a policy course.
Average calss size is about 40.
Meets 3 times/week for one hour sessions.
Each instructor makes up his own exam; each class has its own characteristics, depending on approach of the individual instructor.

COLLEGE OR UNIVERSITY: Riverside City College

FACULTY LIAISON - PROFESSOR: Fred A. Thompson

ADDRESS: Riverside City College
4800 Magnolia Avenue
Riverside, California 92506

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course: 280

TEXTS: Main text: McConnell

COURSE DESCRIPTION: Semester offerings of micro and macro; may be taken in any sequence.
Class size 20-50 students per class.
3 meetings per week.
No teaching assistants.

OTHER INFORMATION: May change text this spring.
Use case studies, problem sets, and computerized games.

COLLEGE OR UNIVERSITY: South Dakota State University

FACULTY LIAISON - PROFESSOR: Felix H. Hsia

ADDRESS: Economics Department
South Dakota State University
Brookings, South Dakota, 57006

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course: 950 per year

TEXTS: McConnell

COURSE DESCRIPTION: Principles I is basically macro
Principles II is basically micro
No teaching assistants, multiple sections shared by staff of the Department
Enrollment breaks down to about 600-650 for Fall Semester, divided into 5 or 6 sections; about 250 - 300 for Spring Semester, divided into 3 or 4 sections; and about 100 in the Summer program, divided into 1 or 2 sections.

COLLEGE OR UNIVERSITY: Stanford University

FACULTY LIAISON - PROFESSOR: George L. Bach

ADDRESS: Graduate School of Business
Stanford University
Stanford, CA 94305

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course: 1200 per year

TEXTS: Bach, Economics
Samuelson
Attiyeh, Lumsden, Bach

COURSE DESCRIPTION: Given four times a year by different instructors.
10 Week Course
Class meets four hours per week in large lecutres.
Teaching assistants have groups of 20 students.
Use programmed learning materials and some computer games.

COLLEGE OR UNIVERSITY: State University of New York at Binghamton

FACULTY LIAISON - PROFESSOR: Laurence E. Leamer

ADDRESS: Department of Economics
S.U.N.Y. Binghamton
Binghamton, New York 13901

NUMBER OF STUDENTS:

Micro Course: 141
Macro Course: 191
Combined Course:

TEXTS: Each instructor may choose his own. Recently have used:
Spencer
Miller
Lancaster
Heilbroner
Gill + Attiyeh programmed text

COURSE DESCRIPTION: Class size: Lecture sections 75-125; discussion sections, 20-35.
Two one-semester courses.
May choose between two forms: two lectures + small discussion section under a professor assisted by teaching assistant section leaders, OR 3 times weekly section of 20-35 students under a professor or a teaching assistant.
Use 4 teaching assistants each semester as section leaders in the large sections; use two each semester as leaders of own sections.
Class meets 3 times per week.

COLLEGE OR UNIVERSITY: U.S. Air Force Academy

FACULTY LIAISON - PROFESSOR: Lt. Colonel Edward L. Claiborn, DFEGM

ADDRESS: U.S. Air Force Academy
Colorado Springs, Colorado, 80840

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course: 1000

TEXTS: Miller, Economics Today.
Supplemented with locally produced readings emphasizing production theory, optimization, defense economics, personal finance, interest and bond markets.

COURSE DESCRIPTION: Each student is required to take 5½ semester hours during sophomore year.
Instructors are military officers with at least a masters degree in economics or management.
Typical class size is 20.
Each class meets 2½ times weekly (3 times one week, 2 times the next).
Students come with one semester of introductory calculus and take calculus concurrently with economics.
No teaching assistnats are used.

OTHER INFORMATION: Each year one section of 20 students uses a different text as an experiment. Because of stringent controls on grade equity, experimentation is limited and strictly controlled.

THIS RESPONSE SHOULD NOT BE CONSTRUED AS A COMMITMENT TO PARTICIPATE BUT, RATHER, AS A REQUEST FOR FURTHER INFORMATION ON THE PROPOSED EXPERIMENT.

COLLEGE OR UNIVERSITY: University of Alaska

FACULTY LIAISON - PROFESSOR: Jerry Rohacek

ADDRESS: University of Alaska- Anchorage CC
2533 Providence Avenue
Anchorage, Alaska, 99504

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course: 300

TEXTS: A series of papers written by the instructor.

COURSE DESCRIPTION: 1 Semester of micro, 1 Semester of macro.
About 30 students/ section
Students are provided a series of papers written by the instructor. They answer a set of "A" questions. In class, a set of "B" questions is covered. These are extensions and applications of the material that was presented in the papers.

COLLEGE OR UNIVERSITY: University of California, San Diego

FACULTY LIAISON - PROFESSOR: Donald V. T. Bear

ADDRESS: Department of Economics
University of California, San Diego
La Jolla, California 92038

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course: 500

TEXTS: Variable

COURSE DESCRIPTION: Two 3 quarter sequence courses of micro, macro, and applications.
One sequence consists of 2 hours lecture plus one hour discussion. Enrollment of 350
Other sequence is micro, macro and applications but has more math. Enrollment of 150

COLLEGE OR UNIVERSITY: University of California, Santa Cruz

FACULTY LIAISON - PROFESSOR: David E. Kaun

ADDRESS: Adlai E. Stevenson College
University of California, Santa Cruz
Santa Cruz, California 95064

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course:

TEXTS:
C.L. Schultze, National Income Analysis
J.S. Duesenberry, Money and Credit: Impact and Control
Council of Economic Advisors, Economic Report of the President
D. Mermelstein, Economics: Mainstream Readings and Critiques

COURSE DESCRIPTION:
Basic lecture course with optional section meetings
2 lectures / week of two hour duration; one section meeting per week of 1-2 hours duration.
Class size varies from 40 - 70.
Utilize the Wall Street Journal in class discussion.

COLLEGE OR UNIVERSITY: University of Hawaii

FACULTY LIAISON - PROFESSOR: Dr. Johannes Overbeek

ADDRESS: Department of Economics
Room 458
2540 Maile Way
Honolulu, Hawaii 96822

NUMBER OF STUDENTS:

Micro Course:	129
Macro Course:	297
Combined Course:	629

TEXTS: Lipsey & Steiner
McConnell

COURSE DESCRIPTION: 3 class meetings per week.
If more than 70 students in the class, the instructor may have a teaching assistant. The class meets twice a week with the instructor and once a week in smaller "lab" sections with the teaching assistant(s).

COLLEGE OR UNIVERSITY: University of Illinois at Urbana-Champaign

FACULTY LIAISON - PROFESSOR: Donald Paden

ADDRESS: Department of Economics
University of Illinois at Urbana-Champaign
Box 111
Commerce Building (West)
Urbana, Illinois 61801

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course: 350

TEXTS: Paden, An Introduction to Economic Analysis

COURSE DESCRIPTION: Use a combination of text, lecture-quiz, seminar, CAI with personalized instruction and programmed learning.

OTHER INFORMATION: The philosophy is that there is no one best way to respond to diverse interests of students.

COLLEGE OR UNIVERSITY: University of Michigan

FACULTY LIAISON - PROFESSOR: Robert S. Holbrook

ADDRESS: Department of Economics
The University of Michigan
Ann Arbor, Michigan 48104

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course: 350 - 600

TEXTS: McConnell mainly.
Lipsey & Steiner
Zusfeld

COURSE DESCRIPTION: Two Formats:

I. one hour lecture, 3 hours section meeting with teaching assistant.
35 students in 10 - 15 sections

II. 3 hours lecture, one hour meeting in section with teaching assistant.
35 students per section in 4 sections.

Traditional approach; few audio-visual aids, TV, programmed texts, etc. are used.
One term introduction with an optional second term for those who wish further work or additional topics.

COLLEGE OR UNIVERSITY: University of Minnesota

FACULTY LIAISON - PROFESSOR: Ed Coen

ADDRESS:

University of Minnesota
Department of Economics
1035 Business Administration Building
Minneapolis, Minnesota 55455

NUMBER OF STUDENTS:

Micro Course: 800 per quarter
Macro Course: 800 per quarter
Combined Course:

TEXTS: Spencer; Lipsey & Steiner, Miller
Reynolds, Microeconomics: Analysis and Policy
Schultze, Setting National Priorities
Coen, Notes for Economics 1002, Parts I and II

COURSE DESCRIPTION: Classes meet three times per week in sections of 60; taught by 2nd or 3rd year graduate student. One lecture per week by Walter Heller to all 800 students on policy issues.
Graduate student instructors make up own exams and homework assignments. Heller gives a mid-quarter and final test.

COLLEGE OR UNIVERSITY: University of Missouri - Columbia

FACULTY LIAISON - PROFESSOR: John Kuhlman

ADDRESS: Department of Economics
University of Missouri - Columbia
217 Middlebush
Columbia, Missouri 65201

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course: 1000 - 1100

TEXTS: Lumsden, Attiyeh, Bach MICRO MACRO PROGRAMMED TEXTS

COURSE DESCRIPTION: 3 lectures and 2 lab sessions; 50 minute classes
Lab sections are 25-30 students, led by teaching assistants. Teaching assistants are discussion leaders and have the exclusive responsibility for the case book materials. They do not give lecturers of their own or more than 50 minutes of quizzes and tests during the semester.

OTHER INFORMATION: "Grabbers" are utilized. A grabber is a short TV cassette tape which is played over the TV system in the lecture auditorium. They are played about 1-2 minutes after the bell rings. Students are quiet when the TV goes on and the light turned down. Each grabber deals with some topic and is intended to lead to further discussion.
"Tele-Lectures" are used from time to time, when the lecturer must be out of town. The lecture is taped and then shown at the regular class hour. The lecture is done as a two-way dialogue with lecturer and a graduate student. It is a good way to present the more technical topics.

COLLEGE OR UNIVERSITY: University of New Orleans

FACULTY LIAISON - PROFESSOR: James J. McLain

ADDRESS: University of New Orleans

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course: 1800

TEXTS: Leftwich & Sharp, Economics of Social Issues
Robertson, American Economic History,

COURSE DESCRIPTION: An introductory course precedes the basic micro-macro course.
Mass lectures twice a week; discussion sections once/week utilizing 8 teaching assistants

OTHER INFORMATION: The Robertson text is to be replaced in the fall of 1975, perhaps by R.L. Miller's economic history paperback or one by North.

COLLEGE OR UNIVERSITY: University of Pittsburgh

FACULTY LIAISON - PROFESSOR: Reuben E. Slesinger

ADDRESS: 313 Mervis Hall
University of Pittsburgh
Pittsburgh, Pennsylvania 15213

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course: 950

TEXTS: Slesinger and Osman, Basic Economics
Slesinger, Today's Economic Issues

COURSE DESCRIPTION: Two lectures weekly of about 150-160 for a term of 14-15 weeks, plus one section of between 20-25 students. Utilizes about 8 teaching assistants. 3 senior T.A.s help in team lecturing; one t.a. helps coordinate the course; other t.a's are assigned to recitation sections.

COLLEGE OR UNIVERSITY: University of Redlands

FACULTY LIAISON - PROFESSOR: Jack Tatom

ADDRESS: Economics and Business Administration
University of Redlands
Redland, California, 92373

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course: 100/semester

TEXTS: Lipsey & Steiner
Miller & Williams, Economics of National Issues
North & Miller, Economics of Public Issues

COURSE DESCRIPTION: Class size averages about 37.
Meet 3 hours per week.
No teaching assistants, but do use graders on "objective" tests.

OTHER INFORMATION: Plan to change to 4 one hour meetings per week in the Spring.

COLLEGE OR UNIVERSITY: University of Texas at Arlington

FACULTY LIAISON - PROFESSOR: Dr. Joan McCrea

ADDRESS: Department of Economics
University of Texas at Arlington
Arlington, Texas 76019

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course: 1250 students per semester

TEXTS: Samuelson
or
Miller
or
Leftwich & Sharp

COURSE DESCRIPTION: There is 1 200-student section; 8 63-student sections; 14 40-student sections.
Two teaching assistants have full responsibility for 4 sections.
Course is given in 2 semesters; meets 3 hours per week.

COLLEGE OR UNIVERSITY: The University of Texas at Austin

FACULTY LIAISON - PROFESSOR: Charles B. Knapp

ADDRESS: College of Social and Behavioral Sciences
The University of Texas at Austin
Department of Economics
Austin, Texas 78712

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course: 5,000

TEXTS: At discretion of professor
Have used Lipsey and Steiner, Heilbroner, Gill

COURSE DESCRIPTION: Two semester macro-micro sequence.
Class meets 3 hours per week. Class size 50-100.
One-half of the classes are taught by faculty members, one-half by teaching assistants.
Teaching assistants have office hours but not individual quiz sections.

COLLEGE OR UNIVERSITY: University of Wisconsin, La Crosse

FACULTY LIAISON - PROFESSOR: Dr. Richard E. Schoenberger

ADDRESS: Department of Economics
University of Wisconsin-La Crosse
La Crosse, Wisconsin 54601

NUMBER OF STUDENTS:

Micro Course:	250
Macro Course:	450
Combined Course:	130

TEXTS: CAI materials in the combined course

COURSE DESCRIPTION: A conventional macro-micro sequence semester course of 3 credit hours is given. Average class size about 40. 6 Student tutors are provided to help those having problems. Classes meet for 50 minutes 3 days/week or 75 minutes 2 days/week.

In addition there was one experimental course of 130 students using CAI materials.

COLLEGE OR UNIVERSITY: Utah State University

FACULTY LIAISON - PROFESSOR: H. Craig Petersen and/or Glenn F. Marston

ADDRESS: Department of Economics
U. M. C. 35
Utah State University
Logan, Utah 84322

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course: 1250 per year

TEXTS: Albrecht

COURSE DESCRIPTION: Two quarter macro-micro sequence.
Classes meet 5 times per week.
One teaching assistant in each of the large sections.
Classes divided into two large sections of 150-250
and two smaller sections of 50-80.

OTHER INFORMATION: Both micro and macro also offered during the summer.
Approach to teaching varies with instructor.
Some use visual aids, computer games, case problems,
and programmed learning.

COLLEGE OR UNIVERSITY: Vanderbilt University

FACULTY LIAISON - PROFESSOR: John J. Siegfried

ADDRESS: Department of Economics and Business Administration
Vanderbilt University
Nashville, Tennessee 37235

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course: 400 - 600

TEXTS: Samuelson or Spencer

COURSE DESCRIPTION: Two options:

One: Large lecture meeting 3 times per week; teaching assistants used for grading; 2 sectiors of 150-250 each.

Two: Small lecture meetings 3 times per week; taugnt entirely by advanced grad students; 3-4 sections of 30 students each.

OTHER INFORMATION: Also have Fels JCEE-Experimental Course
60-90 students per semester.
PSI
Spencer used as text.

COLLEGE OR UNIVERSITY: Villanova University

FACULTY LIAISON - PROFESSOR: Kishor Thanawala

ADDRESS: Department of Economics
Villanova University
Villanova, Pennsylvania 19085

NUMBER OF STUDENTS:

Micro Course: 500-600 per semester
Macro Course: 500-600 per semester
Combined Course:

TEXTS: Undecided

COURSE DESCRIPTION: Two three-credit course: one macro, one micro.
No teaching assistants are used.
Class meets 5 days per week for 105 minutes each day.

COLLEGE OR UNIVERSITY: Wabash College

FACULTY LIAISON - PROFESSOR: William C. Bonifield

ADDRESS: Department of Economics
Wabash College
Crawfordsville, Indiana 47933

Number of Students:

Micro Course:
Macro Course:
Combined Course: 180

TEXTS: Gordon and Dawson last year.
Soule the year before.
Heller plus Elzinga, A Reader this year.

COURSE DESCRIPTION: A one semester course.
30 students per section.
Each section is independent but they try to use the same book.
Some use of simulations and games plus some role playing.

COLLEGE OR UNIVERSITY: Wellesley College

FACULTY LIAISON - PROFESSOR: Helen F. Ladd

ADDRESS:

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course: 300

TEXTS: Samuelson, Economics,

COURSE DESCRIPTION: Two one term courses, macro and micro. Student may take in any sequence, or just one, or both. Receives credit for only one and encouraged to take both. Sections of 25-35 students meet twice a week in 70 minute sessions.
Faculty members give own exams, reading lists

COLLEGE OR UNIVERSITY: Westchester Community College

FACULTY LIAISON - PROFESSOR: Enzo V. Allegretti

ADDRESS: Hartford Hall
Westchester Community College
75 Grasslands Road
Valhalla, New York 10595

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course: 450

TEXTS: Spencer, Contemporary Economics,
Bach, Economics
McConnell, Economics

COURSE DESCRIPTION: Two semester course
Class meets for 3 50-minute periods a week.
Class size averages about 30.
The Macro ecoonomics is taught using the Behavioral Objective system.

OTHER INFORMATION: Interested in participating in any experimentation concerning new techniques.
Located in the county which has the second highest per capita income in the U.S.

COLLEGE OR UNIVERSITY: Wilbur Wright College

FACULTY LIAISON - PROFESSOR: Peter R. Senn

ADDRESS: Wilbur Wright College
City Colleges of Chicago
3400 North Austin Avenue
Chicago, Illinois, 60634

NUMBER OF STUDENTS:

Micro Course:
Macro Course:
Combined Course: 400-500

TEXTS: Samuelson used by half of the teachers;
Others used also PLATO, CAI materials.

COURSE DESCRIPTION: 12 sections of 35 students each.
Class size 35-40.
3 meetings per week; 2 semesters
No teaching assistants used.